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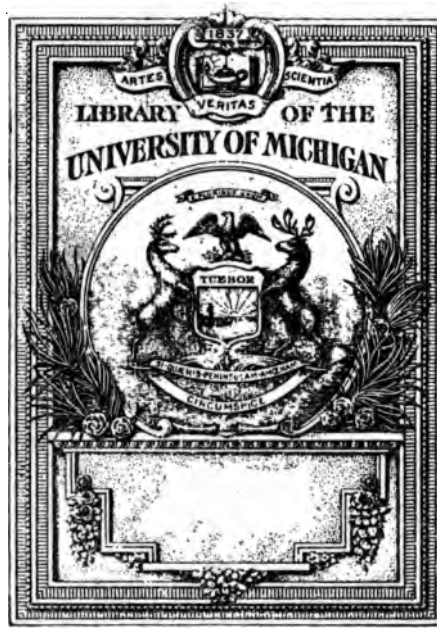
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ARCHIVES OF SURGERY.

BY

JONATHAN HUTCHINSON, LL.D., F.R.S.,

Consulting Surgeon to the London Hospital, and late President of the Royal College of Surgeons.

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CV. A Tongue in Secondary Syphilis.

CXV., CXVI. Duplication of the Skull—Craniopagus.

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CLXI. Hebra's Bubo.

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JANUARY, 1896.

ON EXAMINATIONS AS AN AID TO EDUCATION.*

An Address delivered at the Opening of the Session of the Liverpool Medical College, on Tuesday, Oct. 1st.

MY LORD DERBY, LADIES AND GENTLEMEN,—In searching for some topic which should prove of general interest on the present occasion, I have determined upon that of Examinations. It is one which concerns all present. We have all of us been examined, most of you expect to be examined again, some of us are examiners now, and others will be in the future. A few of us are ex-examiners looking back upon our own careers, and reflecting with much interest upon the whole machinery of modern education, of which examinations now form such a prominent part. I wish to speak not solely of our professional education, but to venture for a moment to glance over a wider field. Examinations are the order of the day, and without much regard of sex or station

* Although the occasion for the delivery of this address was simply the invitation with which my Liverpool friends had honoured me that I should open their session, yet I cannot plead that it was a hasty performance. The subjects with which it deals had for many years occupied much of my thoughts and I was very willing to avail myself of so good an opportunity of giving them expression. The same motives induce me now to endeavour to afford my conclusions a permanent record. I can only hope that I shall not be accused of having overrated their importance.

in life, we now see the youth of the whole community offer itself as so much subject-matter for the examiner's scrutiny. The motives for this vary in different cases. With many it is the necessity of obtaining some certificate or diploma which is essential to success in life, with others merely a desire to be honourably distinguished, and with yet others a conformity with custom and an unwillingness to be in any sense left behind. Various, however, as may have been the motives which have drawn students to an examination room, on one point there will have been uniformity, and it is most important. It is this: that the scope of the examination in prospect has been all-powerful in determining that of the previous course of study. "This I shall want;" "that I shall not be asked," have been the ever-present motives in the selection of subjects for reading and thought. Whatever was deemed not likely to be questioned about has been ruthlessly put aside, whatever its intrinsic interest. The demands of the examiner set the pace of study, and the breadth of the examination determines the narrowness, or otherwise, of the student's education. Such being the facts, and the probability being that they will increase in cogency as years go on, it is obviously of the utmost importance to the community that careful consideration should be given to the perfecting of our examinations. They may become a hindrance to the spread of knowledge and to the development of the mental powers, or they may, if carefully managed, be made a powerful aid to both.

The aim of sound education should be to convey to the coming generation as much as possible of the knowledge already acquired by its predecessors, and in doing this to still leave the faculties untrammelled and the memory not overburdened. The teacher, whilst seeking to create and to satisfy the appetite for knowledge, must above all things be on his guard lest his well-intentioned endeavours may repress it and cause disgust. There will always, it is to be feared, be limits to what can be effected by pass or pluck examinations in creating and fostering the pure love of knowledge. Here and there it may happen that the mind is led to see he attractiveness of some subject which would have escaped

its attention had it not been thus forced upon it, just as a sportsman may see the beauty of a wild moor which he would never have visited but in pursuit of game. In the main, however, it is all the other way, and the fag of preparation, the fatigue of memory, and the innate dislike of giving reasons on compulsion, create distaste rather than otherwise for the subjects concerned. Against this—it is to be feared almost natural result of pass examinations—it is wise to take all suitable precautions. Examinations should be made as little distasteful as possible. To this end it is essential, in the first place, that the element of uncertainty should as far as possible be eliminated. The candidate ought to feel throughout his studies that in presenting himself to an examiner he does that which is equivalent to placing himself on a weighing-machine, and that the verdict recorded will be in exact relation to his deserts. In proportion as the hope is indulged that success may be attained by good luck will thoroughness of study be neglected; and just as it may be possible to suspect that the mood of the examiner, the complexity of a question, or some other accidental circumstance may have influenced the result, will be the disappointment and vexation of a pluck. These considerations bring us at once face to face with details.

It has been, I believe, the steady tendency of all examination boards to dispense with personal contact between the examiner and the examined, and to trust more and more to written questions and answers. To take as an instance our own College of Surgeons; the time is not so very long ago when Sir Astley Cooper and a few of his compeers met together after dinner, and in a very short space of time, and with very little formality indeed, decided the fates of a batch of candidates. If a student were a man of gentlemanly bearing, if he had studied at the proper school and did not display gross ignorance, he was in no sort of danger. The examiner might ask his name and place of birth, and, having learned them, possibly send a kind message to his father or uncle. All this has been changed now, and so much for the better that a feeling akin to reluctance arises in asking you to entertain the question as to whether further improvements

are possible. A large part of our College examination is now in writing; it is divided into various sections, and it occupies probably as many quarters of an hour as the old one did minutes. To be personally known to an examiner is possibly now a disadvantage rather than otherwise. The responsibility for the result is now shared by a dozen or more of conscientious men acting to a large extent in independence of each other, but never singly. Yet, in spite of these improvements, we still hear complaints—sometimes loud ones, and coming from those who have a right to make them—that the scales at Lincoln's Inn Fields do not always weigh correctly. A man may be rejected who was the pride of his teachers, and one may be passed whom they much mistrusted.

That I may avoid the ungracious position of appearing as the critic of any single institution, and at the same time that I may escape the necessity for repetition, I will now venture some general suggestions applicable to all examinations. The personal element, that of the examiner, should be eliminated as far as possible. To this end *vivâ-voce* examinations should as far as practicable be avoided. I have often heard self-confident examiners allege that they could tell better what was in a man in five minutes' conversation than by reading any number of his written papers, and I did not doubt that they thought so. This judgment of men by personal inspection is, however, often most fallacious, and should be permitted only with the utmost circumspection. Most certainly the impression so formed by any single examiner, or any two associated examiners, ought never to be permitted to override that of their colleagues. In other words, the result of the examination ought always to be determined by the sum of the marks gained, and no rejection should take place in consequence solely of the adverse vote of a single table. This applies to all departments of the examination, but with special emphasis to the *vivâ voce*, in which no individual examiner ought to possess the power of rejection. A candidate may be at a great disadvantage in the *vivâ-voce*, especially in the hands of an examiner who thinks it his duty to pin him to some subject in which he

appears weak. The dawning perception of the possibility of rejection may deprive a good man of his resources, and ensure for him a wholly undeserved fate. Another great objection to *vivâ-voce* examination is that the decision must be recorded hurriedly, on the spur of the moment, and that there remain no data in evidence to which to recur. Few indeed are the subjects in which a candidate is not most likely to do himself justice (whether to his advantage or otherwise) in a quiet half-hour with pens and ink. It by no means follows that the disuse of the *vivâ voce* would throw us back wholly on merely verbal questions. There remains the extensive field of objective examination. Under this head we have the identification of specimens and the use of the microscope and of chemical tests, and the diagnosis of disease in the living patient. This kind of examination it is which conduces most of all to sound, matter-of-fact, objective teaching. It is, perhaps, the most important of all modes, and is the one which in the future is destined to receive more and more attention. Although it may be admitted that the diagnosis of disease in patients can seldom be suitably done excepting in the presence and under the supervision of an examiner, yet it is better that the latter should not interfere by question or suggestion, and that the candidate should be permitted at his leisure to write out the opinion which he has formed.

By the term "objective" I mean the inspection, identification, and description of things. Now no one can pass an objective examination unless he has pursued objective methods of study, and this is exactly what we want to induce our students to do. In Geology, for instance, a man could not offhand identify the spine of an echinus or distinguish the shells of a brachiopod from the true bivalve unless he had seen and handled such objects before. If he could not identify the echinus it would be certain that he knew little or nothing practically of the geology of the chalk. So also in Botany; put before a student twenty leaves of common English plants, and let him have ten minutes to name them. Let him know beforehand that such is the kind of knowledge which he will have to display. In pre-

paring for such an examination he will have no alternative but to go into the fields and learn from nature. In our professional examination under the head of objective we include patients, anatomical and pathological preparations, instruments and appliances, the dead body for extempore dissections and operations. I would even venture to add models, good photographs, and drawings. If a student knew that he might be asked to run through Hebra's Atlas, or that of the New Sydenham Society, and give his guesses at the recognition of the plates, he would, as a matter of course, give up an afternoon to the inspection of these Atlases before he presented himself to the examiner. His coach would go over them with him, and they would both of them acquire very useful information as to dermatological diagnosis. It should be, I think, the main object of examinations to encourage students, nay almost to force them, to this kind of objective work. Surely it need not be said that I do not regard plates or photographs as in any way comparable in value with living patients. They are, however, very valuable for the purpose in view, and are much more easily accessible.

We come next to the consideration of papers and of paper setting. Here I have an innovation to suggest. It is that examiners should not be allowed to extemporise their questions, but should be furnished with lists of approved ones. Their selection should be restricted. It is impossible to deny that of those now set many are ill expressed, and some wholly unsuitable. It is no legitimate part of an examination to take the candidate by surprise or confound him with the unexpected. Nor should half of the time allowed be taken up in the effort to understand what the terms of the question are intended to mean. Yet obscurities of this kind are inevitable under the present scheme of question-setting, which involves the preparation of them by men under much pressure of other engagements, and with but little time at their disposal. Questions suitable for examination purposes should be devised at leisure, and expressed in the clearest possible language. Under this plan each separate examination should have its own catechism of questions, to which

the examiner should be restricted. Of this a revised edition should be published every five years. It should be a rule that a third of the questions selected should be of the objective kind, and should necessitate the recognition or description of something. By this precaution the evils of cramming might be forestalled, for the crammer must perforce become the teacher of sound knowledge.

I know that it will be said that to publish beforehand the precise words of all questions that can be asked would lead to cramming up their precise answers, and would put more power into the hands of the grinder. My answer to this is that the student who had learned the answers to all the questions would simply have learned his subject. The questions would be varied in character, many of them would make demands on judgment as well as memory, and they would be so numerous as to be exhaustive in reference to all the more important matters. Why should the examiner fear the coach? Surely he has it in his own hands to devise methods of protecting himself against any meretricious modes of instruction, if any such should be attempted. When I was a student I attended the class-room of the late Mr. Hind, one of the most distinguished coaches of his day. Now the difference between his teaching and that of my school professors was chiefly that he put more energy and point into what he had to say. He taught with zeal and compelled us to listen with attention. His lectures were, of course, merely supplementary to the school course and to home reading. Such, however, was his ability that I remember him with much gratitude to this day, and have an impression that I often learned more from one of his vivid expositions than I could have done from half a dozen of the lectures of my more orthodox teachers. Since then I have done a good deal of coaching myself, and have formed a very strong opinion that the best way to assist a candidate for a pass examination is to give him clear ideas. The number of students who are either really idle or really dull is not great, but the number of those who, for want of guidance as to how to work, fail to gain all that they might from their labours, is very large. Now it is as aids to study that these

books of questions would be most useful. Not only would they place reasonable limitations on the ingenuity of the examiner, but they would throughout the whole of his course give valuable assistance to the student. They would to some extent take the place of the coach, and give to the home student the same sort of advantages that are found in class-rooms. By their aid he would find out where his knowledge was deficient or weak. Definiteness would be given by a well directed question to many a subject which had remained only hazy after a diligent course of reading. It is, perhaps, going a little wide of my topic, but I should like to add that I believe such books would be largely used by amateur students not intending to offer themselves for any examination. They would form excellent guides for self education. It is true that many student's books contain lists of questions which the student may use if he likes. The fact that such have been published is testimony to their value, and I feel sure that such value would be greatly increased if the questions were issued under some authoritative sanction and were known to have been carefully prepared. Let me illustrate my meaning. I am myself much interested in geology. Now if a good question-catechism having the sanction of known authorities were extant, I should buy it; it would be the companion of my reading, and it would doubtless be the means of making me definitely conscious of much ignorance, and thus conducing to its removal.

It will, I hope, be clear from what has been said that examinations may have a double purpose. Not only are they designed in the various professions to prevent the intrusion of ignorant men, and in the general pursuits of life to give to the able certificates of ability which are refused to all others; but they may be made also to greatly assist the purposes of the teacher. The first is unquestionably their primary and fundamental object, but the latter, which may be quite simultaneously kept in view, is perhaps in some respects the more important. Not only do they keep up the standard of knowledge and define somewhat its scope, but *they offer* to the student, if he will but take them kindly,

help of the most invaluable kind. Rather, perhaps, I ought to say that they might be made to afford such help, for it is part of my contention that in this direction they might be much improved. The student should go before the examiner in the same quiet mood that he would go to his master in gymnastics, to show how much he can lift, how high he can jump, and how fast he can run, and he should be enabled to believe that there will be as little room for error in estimation in the one case as the other. From a failure under such conditions he would return to further efforts, disappointed it is true, but with no vexatious sense of possible injustice and of yet future uncertainty mingling with his chagrin and embittering his pursuits.*

One great collateral advantage of systematic examinations of the kind suggested would be that those conducted by different diploma-giving bodies might be brought more nearly into uniformity. The written answers should be preserved, and, in the case of our own profession, ought to be producible at any time on the demand of the General Medical Council. Nor would there be the necessity which is now supposed to exist for the employment of seniors in the profession as examiners. Younger men, having leisure and working under the supervision of an experienced chief, might do the work not only as well, but much better. It must be noted as a distinct drawback in our present system that those employed in it are men whose time and thoughts are

* Might it not be possible to devise some means for testing the correctness of the scales? If there were, as I think there ought to be in all examinations, a permanent president and supervisor, he should make it his duty to take notice of any remarkable discrepancies in the marks given. Thus, if a man does uniformly well at four or five tables, and is recorded as very bad at a sixth, the possibility of error should be suspected. In the case of men who are rejected and come up twice or more, the marks gained on each successive occasion should be compared. If the difference be only moderate it is probable that the record is a just one, but if it be very large some accident may be suspected. Thus, as an example, if a weight of fifteen stones being demanded, a man was found on three successive occasions to touch only thirteen or fourteen, the presumption would be that he was accurately estimated; but if a man weighing only eleven on one occasion was found six months later to have gone up to nineteen, some error has occurred. It is not possible to put on flesh or even fat at that rate. If such startling discrepancies should occur often, it would certainly imply that the method of examination was faulty.

very fully engaged in other matters, and who come to their examination work already wearied with that of the day. A never-failing answer to all projects of reform is that the time of the examiners must be considered, and proposals which would involve either extension of time or addition of work are sure to encounter determined opposition. The machinery of examinations will, however, never work satisfactorily until considerations of time and trouble are put wholly aside. Nothing is more essential to just conclusions than that the process should be conducted with leisurely quiet; nor, let me add, should it be in the hands of those who, in consequence of frequent changes and short periods of office, are more or less inexperienced. It is the fashion to hold that any one who knows his subject can examine in it, but in truth it is not so, and the art of examining is one which, like other arts, needs to be learned. The man who has been some years at work makes, I feel sure, other things being equal, a far more trustworthy examiner than a novice. To some extent, and especially in some departments, examining should be a profession in itself.

Examiners should, I repeat, be men of leisure, neither too young nor too old, and above all, they should be men of patient minds. If ever examinations are to assume their proper place as aids alike to the student and the teacher, they must become more detailed, and necessarily take up more time. Any plan which confers examinerships on men who have attained eminence in their several professions is open to exception on the score that such men will usually be fully occupied in other pursuits. They will consequently prefer to be allowed to estimate candidates personally, since that is the quickest and least laborious way. Above all, they will be steady opponents to all reforms which are likely to increase the demands upon the examiner's time or give him additional trouble.

There is also another good reason why men of this class ought not as a rule to be engaged in examining. They are worthy of better employment. The time and energies of a man who possesses original ability ought not to be taken up in the laborious, but monotonous and in some sense unpro-

ductive, work of an examiner. As one grudges such men as Mr. Lecky and Mr. John Morley to the work of the House of Commons, so one cannot but regret that the time of Dr. P—— and Mr. H—— should be wasted on the task of gauging the attainments of students. There are those who could do this latter as well, possibly even better, than they, and who are far less well fitted for original investigation. We ought not to put racehorses to the plough. Acting on this rule, some of the wisest and best in our profession have in the past declined to undertake the burden of examination work. They did right. If, however, those of highest qualifications should refuse to be elected as examiners, the result under our present system, which leaves so much to individual judgment, is obvious. Under the plan which I propose, the need for judgment and skill on the part of the examiner would be lessened. What would be wanted would be patience and leisure, with, of course, knowledge of the subject. The questions would be prepared to hand, and the *vivâ-voce* part would consist of little more than the supervision of the candidate during his attempts at the recognition of the objects submitted to him.*

It is to be freely admitted that examinations conducted in the manner proposed would lose much in picturesqueness. They would gain, however, in security and in justice. I am mistaken if they would not gain also very much in the assistance afforded to the students, their teachers, and to education generally. It is of course open to those concerned in giving

* A practice which must be mentioned as being eminently conducive to inequality and uncertainty in the results of examinations is that of allowing candidates to pick and choose amongst the questions proposed. This is probably designed as a kindness to the examinee, but it is often quite the reverse. Let the questions be numerous, brief, and explicit, and require answers to every one of them. As the terms of them all will have been known beforehand there will be no sort of hardship in this. It will save the candidates' time which is now taken up in the preliminary task of selection, and it will place all on an equal footing, thus enabling the examiner to judge better of comparative merit. If a candidate be encouraged or allowed to shirk some of the questions, is it not an admission that some of them may be too intricate or the whole of them pitched in too high a key? These remarks are especially applicable to diploma-examinations. There can be no good reason why a candidate should be allowed to plead ignorance of certain subjects when it is most important that he should know the whole.

diplomas to say that it is no part of their duty to attempt to share in education, and that their vocation begins and ends with the determination as to who are and who are not worthy to receive the qualifications which they confer. Admitting that this is their primary function, and one to which all else should be subsidiary, it may yet be contended that as there is no sort of antagonism between the two, the wider conception of their calling is the more commendable one. The scope of knowledge is now so vast and the difficulties in its attainment so great that we cannot afford to neglect any source of help. We cannot afford to allow that the examiner shall do anything less than his very best to assist the efforts of the teacher.

Now, regarding examinations from the standpoint which has been suggested as part of the educational scheme, and having secured for them the closest possible adaptation to what is reasonably practicable, it may be hoped that they would soon come to be regarded with favour by all concerned. We should cease to hear of "the harass of examinations" and complaints as to their too great frequency. It would, indeed, be in the interests of the student to make them very frequent, for they would be of more assistance to him than many lectures.

I am aware that there are differences amongst those well entitled to their opinions as to the advantages of subdivision in examinations. It is sometimes sought to throw mild ridicule on divided examinations by speaking of them as "taking your dose in teaspoonfuls." It is thought that by allowing a man to come up in one subject at a time you encourage cramming and voluntary forgetting. I may confess that my own impressions are very strongly in favour of subdivision. In their favour it may, I think, be urged, in the first place, that a much more detailed and practical knowledge may be suitably exacted at a special examination than could be demanded at one which included a great variety of topics. If, for instance, a candidate were allowed to present himself whenever he liked for his examination in ophthalmic medicine and surgery, and to take that subject by itself, he would certainly get it up in a very different fashion from what would probably be the case if he were

only going to encounter the risk of a stray question or two as part of an examination in general surgery. He would, in all probability, have attended as an eager and attentive learner at some special institution. Are we to believe that he would forget what he so learnt sooner than he does the scraps of book-information with which, but too often, he is obliged to be content under our present system? He would know well that at such a special examination a much higher standard would be demanded. Having obtained his certificate for eye disease, he would proceed with cheerfulness and with a less burdened mind to other subjects. Undoubtedly much will be forgotten. That is inevitable under every plan. It is not, however, so much the mere change of the attention from one topic to another which tends to obliterate knowledge as the fact that the thing had never been really learned at all. We all remember Dr. Johnson's retort to the man who pleaded that he had "forgotten his Greek."

In support of the principle of divided examinations it may also be urged that it would permit of the restoration of certain subjects, such as Botany and Zoology, which have been for well-known reasons left aside, and even of the insertion of others. I am here possibly on delicate ground. Permit me to say, however, that I do most certainly hold that a medical man ought to be a well-educated gentleman, and that he ought not to wholly omit, from his training at any rate, an elementary knowledge of those branches of natural sciences which are cognate with his special calling. It is quite true that it is no longer necessary to have studied botany in order to prescribe drugs; but there are other and far more cogent motives for its pursuit. As a branch of the great science of biology it presents us with lessons which are full of interest and instruction. The same is to be asserted of zoology, and with perhaps only little less emphasis of geology, climatology, and some other topics. The attainment of knowledge in all these is not difficult if it be sought in the right way. To that end divided examinations—not perhaps compulsory as to the diploma, but resulting in certificates which should be contributory—would be of great assistance.

The advocates of inclusive or comprehensive final examinations—and amongst these are, I believe, some of our leading authorities on education—urge with plausibility that they are better tests of a man's memory and mental powers. To some extent this may be admitted, but their superiority in this respect is probably very limited. By no means can it be alleged that those whose memories have sufficed to carry them through an examination in half-a-dozen different subjects, on one and the same occasion, will subsequently retain the knowledge which they then displayed. A diploma or certificate is, unfortunately, no talisman which will secure us against the treacherous influence of the world's pursuits in obliterating the record of the labours of our student days.

Some confession of weakness on the part of the present system is, I think, to be found in the limitations which it has been found needful to impose upon the scope of these many-subject examinations. Students are informed beforehand that only such and such topics will be taken. This is, perhaps, not done so much in our medical examinations as in those of general education. Thus, in botany a candidate at South Kensington is informed that fourteen natural orders only will be questioned on, and that he need not know anything of Papaveraceæ, for example, and a host of others. At Cambridge he must add Papaveraceæ to his repertory, but may still leave aside almost all the rest. At most of our examinations the important sub-kingdom of Fungi may be left out of consideration altogether. I mention these merely as examples; the same principle will be found to pervade other departments, and the result is a sort of patchwork education, the incompleteness of which is much to be regretted. Our children are taught one reign in English history, for instance, or one short period in that of Greece or Rome, and do not obtain, as I think their forefathers did, a general, though it is to be admitted a superficial, view of the whole.

Modern education, in its zeal to avoid the charge of being superficial, incurs, as it seems to me, that of being merely *frag*mentary. It aims at thoroughness, but is obliged at

once to admit that it can attain it only in certain subjects, which, compared with the sum of human knowledge, are but few and small. Let those of us who advocate wide study, and as a consequence wide examinations, face at once this charge of encouraging a merely superficial acquaintance with things. There is much to be said in its favour. Excepting a very few of us, we are all mere smatterers as regards almost all that we think we know. It is not possible to be otherwise excepting at the cost of being wholly ignorant in many directions, and as regards fitness for the affairs of life, better by far a general acquaintance with all that is around us, though it be not very deep, than slices of profound knowledge placed sandwich-wise between thick layers of utter ignorance. In the medical profession more especially is this alternating method of education to be shunned. We cannot afford to allow attainments in one direction to counterbalance vacuity in another. Hence I think a very strong reason exists for allowing our students to subdivide their studies and take one at a time, with the understanding that they do that thing, for the time, well.

I must now, Mr. President and gentlemen, attempt a brief summary of what I fear has been a very discursive address, and then conclude. If I have given to any the impression that I am in favour of increased stringency or of increased leniency in medical examinations let me hasten to remove that idea. My plea has been rather that our examinations should become more and more an integral part of our educational system, and that by making them more exact and more definite we should enable them to better assist the labours both of teacher and student. The latter especially should be brought to feel that his examinations, so far from being sources of harass and worry, are really his best friends, in gauging his attainments, and at the same time guiding his work. I have advocated frequent examinations but at the same time have urged that the student should be allowed to take one thing at a time, and thus, as it were, to have the pleasure of counting the milestones in his journey, and of feeling sure that he is approaching its end. With the hope

of reducing to a minimum the uncertainties of examinations I have ventured to suggest some curtailment of the privileges of examiners, more especially that they should no longer be invited to exercise their ingenuity in devising extempore written questions, but should have supplied to them carefully prepared lists of suitable ones, which lists should also be accessible to the candidates. If on some matters I have ventured upon detail which may be thought better suited for a board of examiners than for the present audience, my reply is clear. It is time that the public—in the case of our medical examinations I mean, of course, the professional public—should interest itself in this matter. It is one of the widest possible importance, and for reasons which need not now be mentioned, it is one in which only reforms of a certain class should be looked for from within. No one conversant with human nature will expect from those actually holding office reforms which might be thought to reduce the dignity of that office. As to *vivâ-voce* examinations, I have suggested that they should give way to written ones. Surely we are behind the day when we keep up the custom of putting specimens into the hands of nervous students and demanding impromptu recognition, when with less expenditure of the examiner's time a quiet hour with half-a-dozen such preparations might be allowed. Who can doubt as to which would best test the pathological knowledge of the candidate? It is the same with the diagnosis of disease in patients.

Many of you listened not long ago to a very able address from Mr. Mitchell Banks. He took for his text "the Overcrowding of the Profession." With most of what Mr. Banks told you I thoroughly agree. More especially we are entirely at one in his main conclusion; which was, that the safety of the profession lies, not in new legislation of any kind, but in its own examinations. His chief proposal was to increase the stringency of the matriculation, and thus prevent, once for all, the entrance of the illiterate. In what I have said to-day I have taken up the same idea, but have carried it a little further, suggesting that arrangements should be made throughout the whole course of study which should en-

courage the industrious and eliminate the incompetent and idle. The key to the whole situation is in the hands of the examiner. Curricula of study are worthless without his aid, and certificates granted without examination are of no avail whatever. If our examinations were developed we might almost wholly dispense with both curricula and certificates, and leave the student, almost at his free will, to choose and to change his places of study. Once make our examinations trustworthy and thorough and we need no other safeguards. Amongst those who hear me I doubt not there are representatives of different interests in this matter. The student has been too much accustomed to regard his examinations as bores, and looks upon any suggestion which he thinks would make these tests more stringent as intrusive and uncalled-for. Paterfamilias, who has sons to pass, takes the same side, with perhaps added warmth, on account of a keen recognition of the probable increase of cost. Striving practitioners who have neither sons nor nephews are, on the other hand, willing enough that the portals of the profession should be more strictly guarded. To one and all let me earnestly say that we are in the same boat. The student of to-day will be the practitioner of to-morrow. Our common aim should be the improvement of medical education. It is probable that the supposed evils of overcrowding in the profession have been exaggerated, and I am sure that none of us would like to restrict, were it possible to do so, the number of students to that of the supposed needs of the public for medical practitioners. We put a higher estimate on the value of medical training, *per se*, than would allow of our desiring any such protection. From men educated in the schools of medicine have sprung naturalists, men of science, explorers, and those foremost in every branch of knowledge. This has been our boast in the past. Let us take care that we do nothing to frustrate it in the future. Let us put no artificial impediments in the way of entrance to our ranks. Let us, however, at the same time, by careful attention to the details of our examinations, make sure of this—that, if we are to be overcrowded, it shall be by well-educated men.

JOHN HUNTER ON "DECAY OF THE TESTICLE."

(WITH AN ADDITIONAL CASE.)

IN Vol. VI. of ARCHIVES, at page 100, I have recorded some cases of absolute atrophy of the testicle following a form of acute orchitis which is probably neurotic in its origin. Recently another very definite example of this condition has come under my notice. The affection is perhaps of interest chiefly as an illustration of pathological possibility. I am not acquainted with any quite parallel occurrence in other organs—that is, of the entire disappearance of a gland by atrophy after acute inflammation. The testis may shrink somewhat after gonorrhœal orchitis, or after inflammation from blows, but in the cases to which I now refer, it is not a question of mere shrinking, but of atrophy to such a degree that the gland can scarcely be found, and is perhaps not larger than a pea. When I wrote on this subject a year ago, I had forgotten a paper by John Hunter with which I had formerly been quite familiar. It occurs in his Treatise on the Venereal Disease (page 311), and is entitled, "Of the Decay of the Testicle." Hunter records several cases in detail, and his editor, Sir Everard Home, adds, in brief abstract, five others. The subject is of such interest that it seems worth while to quote the thoughtful remarks with which the cases are preceded, and also to give the chief facts as to the cases themselves.

"OF THE DECAY OF THE TESTICLE."

"It would appear, from some circumstances, that the parts of generation are not to be considered as necessary parts of the animal machine, but only as parts superadded for particular purposes, and therefore only necessary when those particular purposes are to be answered; for we may observe that they are later of coming to maturity than any other parts, and more liable to decay. Thus far in their natural properties they are different from most other parts of our body, the teeth only excepted, which are similar in some of those circumstances.

"*The testicles* appear to be more subject to spontaneous disease than

any other part of the body; but what is the most singular thing of all is *the wasting of those bodies.* One or both testicles shall wholly disappear, like to the thymus gland, or membrana pupillaris, etc., in the infant. This we do not find in any parts of the body which are essential to its œconomy, excepting the parts are of no further use and might become hurtful in the body, as in the instance of the membrana pupillaris. But the testicles do not undergo this change as if in consequence of an original property stamped upon them, as is the case of the thymus gland, whenever the age of the person is such as to render them useless, but are liable to it at any age; and therefore the disposition is in the testicles themselves, independent of any connection with the animal œconomy. An arm or leg may lose its action, and may waste in part, but never wholly.

"Testicles have been known to waste in cases of rupture, probably from the constant pressure of the intestine. Mr. Pott has given us cases of this kind. I have seen in the hydrocele the testicle almost wasted to nothing, probably from the compression of the water; but in all these the causes of wasting are obvious, and would probably produce similar effects in other parts of the body under the same circumstances; but a testicle, without any previous disease, wastes wholly; or at other times it inflames, either spontaneously or from sympathy with the urethra, becomes large, and then begins to subside, as in the resolution of common inflammation of the body, but does not stop at the former size, but continues to decay till it wholly disappears. The following cases are instances of this."

The following are abstracts of the eight cases given by Hunter and Home.

CASE I. A gentleman (age not stated) presented himself to Hunter in whom there was "no appearance of a testicle" on one side. His sexual function was as good as ever. The history which he gave was that the testicle had wasted after inflammation in connection with gonorrhœa, nine years before.

CASE II., communicated to Hunter by Dr. Nanfan. A young man of eighteen, who had never had any venereal complaint, suffered a violent attack of orchitis (left) with great pain and swelling. The swelling subsided after six weeks, but atrophy followed, until nothing more remained than what seemed part of an indurated epididymis not bigger than a horsebean. Twenty months later the other testis was attacked in a similar manner, and in spite of much treatment a like result followed, and the testis "continued to decrease until not a vestige was left."

In this case Mr. Adair and Mr. Pott took part in the consultations. It will be seen that the patient's age and the

definite absence of gonorrhœa leaves it a not improbable supposition that the orchitis was neurotic and in association with masturbation or emissions. It is the only case with which I am acquainted in which both testes underwent atrophy.

CASE III., communicated to Hunter by Dr. Cotham, of Worcester. A young man of sixteen was seized with coldness and shivering, which lasted three hours. During it his pulse was so small that it was difficult to count it. The rigor was followed by fever, for which bleeding was resorted to. There now occurred excruciating pain in the loins and left side of belly, passing down into the scrotum. The inguinal region and the testis became swollen. In spite of thirteen blood-lettings and the free use of antimony, etc., the fever continued, and the testis became swollen to the size of an infant's head. Dr. Cotham believed that suppuration had occurred, and wished to make an incision, but could not get the patient's consent. Ten days or so after the first another severe rigor occurred, but without increase of pain. It now looked as if the supposed abscess would burst spontaneously. Gradually, however, the swelling subsided, and the testis was left "the size of a hen's egg, and as hard as scirrhus." Subsequently it wasted, and at the end of a year nothing of it could be detected except "a confusion of loose fibres." Some years later, "of the testicle there was not the least vestige." The patient was then in good health, married, and the father of five children.

This case is remarkable on account of the severity of the early symptoms, and the statement that the pain began in the groin and descended to the testis. The great size attained by the swelling is also noteworthy, together with the supposed indications of abscess. The case which I shall presently relate may perhaps bear upon this latter point, for in it fluid was supposed to be present, and a distinguished hospital surgeon introduced a trocar, with the result, however, that nothing was obtained. The youth of the patient again favours the suggestion that masturbation may have been the exciting cause.

The following five cases are added to Hunter's chapter by Sir Everard Home.

CASE IV. A gentleman had orchitis after the use of a bougie for stricture. The testis subsequently diminished until it was extremely small.

CASE V. A gentleman bruised one testicle in riding. It swelled to a very large size, and subsequently diminished until "it entirely disappeared."

CASE VI. A gentleman was violently lifted by his hands, with the result that he had immediately a violent pain in the left groin and swelling of the right testicle. The latter subsequently atrophied to the size of a pea.

CASE VII. A young man of eighteen had inflammation of one of his testicles, after which it diminished to one-fourth of its natural size.

CASE VIII. A man aged 20 had experienced during eight years successive attacks of pain and swelling in the right testis and cord. At length a more violent attack was attended by severe pain and great swelling. He was confined six weeks by the attack, and after it the gland diminished to the size of a horsebean. The other testis had also been occasionally swollen, but without any subsequent atrophy. This patient had experienced during the attacks a certain amount of pain about the neck of the bladder, especially during micturition.

It is to be regretted that Sir Everard does not in three of his cases give the age of his patients. The two in which the age is stated were under twenty, and one of these had been liable for several years to slight attacks, and in both glands. The suspicion of masturbation becomes strong in this case. It does not appear that in any of this series of cases the sexual habits were made the subject of investigation. The last case is important, as showing that the slight forms of orchitis, not followed by atrophy, may occur under similar conditions. It seems highly probable that the risk of subsequent atrophy is in close relation with the severity of the inflammation. Two of Home's cases are not examples of absolute atrophy, but only of considerable diminution in size.

I will now narrate the case which has recently come under my own notice.

A business man of extensive attainments consulted me on account of the fear of impotence. He had for two years observed continence as regards women, but with "a tremendous struggle." He had been accustomed to stick pins into his skin, and practise other modes of torture, in order to divert the sexual orgasm. His reason for continence was that he was devotedly attached to a woman whom he could not marry, but in whose society he was much thrown. In early life he had masturbated to great excess, and subse-

quently had indulged freely in sexual intercourse. During the continent period he had been troubled by very frequent emissions during sleep, but these had latterly become less frequent and insignificant in amount: hence his alarm. He had found, also, that his sexual excitability was becoming much less than it had formerly been, and he thought that his memory and imaginative faculty were both failing him. On examining his genitals I found that the left testis was quite atrophied, being represented only by a little body not larger than a pea. The other testis was rather small and decidedly soft. He had no varicocele. I asked as to the cause of the atrophy of the left gland, and received the following statement. A very severe attack of orchitis had occurred at the age of 18, without known cause. This was during the masturbation period, and before he had had intercourse with any woman. He admitted, in answer to a definite question, that he believed it was induced by an act of masturbation. The testis swelled so much that abscess was suspected, and a consultation with a distinguished hospital surgeon was had. This gentleman put a trocar into the swelling, and was greatly surprised not to obtain any fluid. The swelling of the gland slowly subsided, and absolute atrophy, as already described, followed. The shrinking of this gland did not appear to have in any degree diminished sexual appetency, for subsequently its subject indulged very freely with women, and continued to do so until the attachment mentioned above induced him to resolutely abstain.

From what this patient told me, it was clear that he had been from a boy remarkably susceptible as regards sexual excitement. It had been his plague through life, and now, at the age of 30, there appeared reason to fear that it might fall as much below what he desired as it had formerly been in excess. As regards the atrophy of the testis, it will be seen that it occurred under the most usual circumstances —that is, to a young man, and wholly unconnected with natural venery or gonorrhœa. The acuteness of the orchitis and the simulation of abscess were also in precise conformity with what has been noted in other cases.

FRAMBŒSIA CROMWELLIANA.

"There is a disorder in this country and in Ireland called the Sivvens, which is a true species of Frambœsia, but the symptoms are not alike in all respects. The 'Sivvens' is an Erse word for raspberry, because, in very advanced states of the disease, certain spongy excrescences break out in various parts.

"As this disorder was first brought to the Highlands of Scotland by the Protector's soldiers, I beg leave to denominate it 'Frambœsia Cromwelliana.'"

THE extract given above is from the pen of Dr. W. Wright, F.R.S., a Scotch physician who had practised in Jamaica. He was therefore familiar with Yaws (Frambœsia) in its native haunts, and also with what was then in Scotland called Sivvens. His testimony that the latter was a true species of "Frambœsia" is consequently of value. Now it has been long ago admitted, as beyond doubt, that Sivvens was nothing but syphilis. It was syphilis spreading for the most part by non-venereal contagion, amongst peasants who lived in crowded hovels and who habitually neglected their ailments until they became intolerable. It becomes, therefore, of much interest to inquire what were the symptoms in Sivvens which led many observers, of a half-century ago, to declare that it was distinct from syphilis. Such inquiry may perhaps throw light on the cognate malady, Yaws, concerning which many observers still allege that, although it resembles syphilis, it is yet a distinct disease. It will also be of interest to ask in what Sivvens differed from Yaws.

The absence of any true chancre, and often of any primary sore as the first symptom, was one of the foremost characters by which Sivvens was distinguished. This negative fact was, however, the result of imperfect observation, and in the main resulted from the circumstance that the disease

was usually acquired by accident, and by the infection of some part of the surface other than the genitals. Now erratic chancres very often display no distinctive characters, and are very often wholly overlooked. It is only amongst the poor and self-neglectful that any extensive spreading of syphilis by other methods than venereal congress can be expected.* With ordinary care and attention to cleanliness, such infection is very unlikely to happen. All the usages of a more advanced civilisation are hindrances to such modes of spreading. In crowded cottages, however, where children and adults sleep together, use the same towels and drink out of the same cups, such accidents are very likely to happen if once syphilis enters a household. They were common in Norway up to a very recent period. It is only the rarity of syphilis in most peasant populations which seems to prevent them. The advent of soldiers, if long continued, is almost certain to introduce the disease, and that, too, under conditions which make its spread in families almost sure to follow. Even the exceptional morality of Cromwell's Puritan army does not appear to have wholly prevented this result. Epidemics of syphilis begun in this way, and occurring in a virgin population, are apt to be unusually severe.

We may hold, then, that the family, or epidemic, prevalence of such a disease as Sivvens (= syphilis) may be explained by reference to the social condition and habits of those affected. There next arises the question as to why syphilis under such circumstances should tend to assume the frambœsial form. We have seen that the word Sivvens means in the vernacular "raspberry-like," and is, therefore, synonymous with "Yaws" and "frambœsia." We may assume, then, that the observation of those concerned did lead them independently to imagine some resemblance between the eruption produced and the fruit in question. That there was a good deal of exaggeration both as to the closeness of the resemblance and the frequency of the occurrence of these granulation growths

* As long ago as 1805, Dr. McLeod, in an Inaugural Dissertation, called Sivvens "Insontium Syphilis."

is highly probable. Many authors, indeed, expressly state, both as regards Yaws and Sivvens, that ulcers, and sores the description of which would fit well with rupia, were more common than the fungating growths. Nor is it without a certain degree of probability that neglect of early treatment and inattention to the skin in general might tend to increase the number of cases in which syphilitic eruptions would take on the framboesial type. That type is not wholly unknown in English practice at the present day. Although we do not now diagnose as "framboesia" any group of syphilitic eruptions, yet not very unfrequently in neglected cases we recognise a tendency to that form. One very remarkable example of this I have recently described. The formation of sores at the angles of the mouth attended by excrescences is perhaps its most frequent manifestation. The younger the patient attacked the greater probability that there will be tendency to papillary outgrowth. In the epidemics of Sivvens many children suffered, and all were in the early stages neglected, and thus I think we may explain the greater frequency of the raspberry type of eruption.*

None of our atlases of skin disease contain any good portrait of a generalised syphilitic eruption of a true framboesic type. None of the older writers who wrote on Sivvens or Radesyge, nor even of those who have treated of Yaws itself, have given us a good pictorial representation of such a state. I will repeat, then, that we may fairly doubt whether in the epidemics of Sivvens, or even in cases of Yaws in our own day, such eruptions have been other than rarities. When the Erse peasantry named syphilis Sivvens, or "the raspberries," they probably had taken note of

* Dr. Frank, of Vienna, who visited Edinburgh in 1802, saw some cases of Sivvens in the hospital there. In referring to one, in the account of his travels, he took occasion to record an outbreak of the disease which had occurred in Dalmatia, and concerning which an official report had been prepared by Dr. Cambieri, of Fiume. Thousands had in the first instance been attacked. Dr. Cambieri recognised it as Sivvens, or epidemic syphilis. Isolation hospitals were established in the affected districts, and treatment by corrosive sublimate exclusively was adopted. In the course of three years the disease was almost exterminated. Much information respecting this and similar endemic outbreaks of syphilis will be found in Hirsch's "Handbook," vol. ii. p. 92, New Sydenham Society's Translation.

a conspicuous but not a very frequent character of the disease.

The Radesyge of Norway, for which, during the latter part of the eighteenth century, special hospitals were required in Bergen and other towns, was also epidemic syphilis. It ceased to prevail as soon as its true character was recognised and proper precautions and suitable treatment adopted. Syphilis, however, still lingers as a family disease amongst the peasant population of Norway, or at any rate it did so when I visited Norway in 1870. I was then assured by Professor Boeck and many other surgeons that it was no very uncommon thing to have to treat grandparents, parents, and children in one group for syphilis acquired, in most instances, by non-venereal infection.

Dr. Wright, in the essay from which I have quoted, does not give us the diagnostic characters which, as he believed, separated Sivvens from syphilis, and, as has been seen, he admitted that Sivvens was a form of Frambœsia. He enters at considerable length into the diagnosis between Sivvens and Yaws, and in so doing gives us incidentally important insight into the creeds of the day. Sivvens, he says, attacks the throat first, Yaws not until after some lapse of time. Sivvens rarely affects the bones, Yaws always, unless well managed. Sivvens is highly contagious, Yaws is contracted only by inoculation. Sivvens is cured early by mercury, but mercury in Yaws is pernicious. In healthy constitutions, "Yaws will go off in time, but if speedy and effectual means be not used in Sivvens the patient will infallibly be destroyed." Such are some of the alleged differences, and to them may be added that "the eruption of Sivvens smells offensively, whilst that of Yaws has no odour," and that the latter is not attended by scabby spots, ringworm-like tetters, or deep ulcers, all of which are often seen in Sivvens.

Although I am in the present paper treating of Frambœsia Cromwelliana, and not of the tropical Frambœsia or Yaws, yet I cannot forbear to quote here a paragraph in which Dr. Wright sums up his reasons for denying of the latter, as he did also of the former, that it is syphilis. In both instances it was, as I think, clear that a very imperfect acquaintance

with the natural history of syphilis, especially in its non-venereal form, led to the confusion.

Dr. Wright's words are, "Several authors have spoken of the yaws and syphilis as different modifications of the same thing. Whoever compares the account we have given will find them widely different. It is true that yaws affects the bones, the nose, and the palate, like syphilis, and admits of a similar cure; but in syphilis there are neither eruptions nor fungi, as in yaws, except on the privities, and then only in form of warts. Yaws attacks the same person only once in his lifetime, and we all know that lues venerea may be and is contracted repeatedly."

The inference which I have desired that the reader should draw from the above remarks has, I hope, been made clear, but in case it has not, I will here state it explicitly.

Framboesia Cromwelliana (sivvens) is now acknowledged to have been nothing but a severe form of syphilis spreading independently of sexual intercourse. Precisely the same kind of reasons were formerly used to prove its non-identity with syphilis which are now employed to prove the non-identity of tropical framboesia (=yaws). It is probable that these reasons are of no more real validity in the latter case than they have proved in the former.

We may ask in reference to these maladies whether it is desirable to permit the use of the word "Syphiloid"? It is one employed by recent writers on yaws, and even (though evidently with much hesitation) by Dr. Hirsch himself. It is now admitted that if yaws be not syphilis it is "syphiloid," that is to say, a malady very like it, though due to a different poison. So long as sivvens, radesyge, and some other locally endemic maladies were held to be other than syphilis, they too were classed as "syphiloid." One writer has even suggested that there may be family groups of diseases, distinct from each other, of which syphilis may be the type example, but still only one. The reply, so far as I understand the question, must be that there is no such group, and that syphilis stands alone, and that the specific poison which produces it produces also all the others which conform to its type. Yaws is indeed almost the only malady which keeps

the field, and as regards it the contest cannot, I think, last much longer. It is obvious that the question will be settled at once if it be proved that the poison of yaws can produce syphilis. If inoculation experiments are deemed unjustifiable, we may yet trust that some careful observer will before long be able to adduce from the accidents of practice evidence which shall be conclusive. I am by no means sure that such facts are not already on record.

ON SPONTANEOUS OBLITERATION OF THE FEMORAL AND OTHER LARGE ARTERIES.

CERTAIN facts which have come under my observation have suggested the belief that obliteration of the femoral artery (one or both) may occur under unexplained conditions, and may lead to very puzzling symptoms. What little search I have been able to make in the writings of others has not revealed much that is of clinical value on this subject, and it seems, therefore, worth while to ask special attention to it. In my Bradshaw lecture, 1889, I made reference to two remarkable cases—one recorded by Sir William Gull and one by Sir William Savory—in which the large arteries of the upper extremities and head and neck were extensively occluded. In neither of these cases was there any explanation of the cause of the condition. The editor of the article in "Holmes' System" speaks of the latter case as an example of congenital disease, but this was not the view taken by Sir William Savory when he published the case.* Sir William Gull's case was accompanied by an illustration showing the mouth of the innominate smoothly closed and the trunk itself a solid cord. I have not been fortunate enough to find any parallel cases in which the large vessels of the lower extremities were occluded. The cases to which I now refer have nothing to do with calcareous changes, nor do they occur exclusively in old age.

CASE I.—*Spontaneous obliteration of one common femoral artery after exposure to cold—Gangrene of toes—Amputation—Renewed Gangrene—Second amputation—Gangrene and third amputation—Recovery.*

It is many years since I published the particulars of the case

* This case is recorded in the thirty-ninth volume of the Medico-Chirurgical Transactions. All the main arteries of both upper extremities and of the left side of the neck were reduced to solid cords. There was destructive ulceration of the scalp and eye, but no gangrene of the digits.

There were facts in the history which made it not improbable that the arterial obstruction had begun in the abdomen, for in the first instance the left limb had been more affected than the other, and the circulation in it was unquestionably still very feeble. It will be seen that some of these facts correspond most remarkably with what had been noticed in the case which I have next to narrate.

It is recorded that, in the case just adverted to, the indications of defective circulation in the feet had been referred to the nervous system, and diagnosed as Raynaud's phenomena. Their true nature became at once evident when the groins were examined. In my next the simulation of nerve disease had been yet more deceptive. The patient brought with him a certificate that he was suffering from double sciatica, and that he might be expected to recover perfectly. There could, however, be little or no doubt that the pain which he suffered was caused by want of blood supply to the muscles. The cases well illustrate the importance of systematic examination of the groins.

CASE III.—*A case in which no pulsation could be detected in the femorals or in any artery of either lower extremity.*

The subject of this case was an officer in the army, aged 40, who had lived much in hot climates, but who had in the main enjoyed good health. It was reputed that he had had syphilis, but the evidence was defective, for he had never had a chancre nor any eruption. Loss of hair had been the only symptom. He had nevertheless taken much medicine, chiefly iodides. For several years before I saw him he had been the subject of what had been called "sciatica" attended by peculiar cramps. This had begun in the first instance in the left limb, but latterly had affected chiefly the right. It had been followed by inability to walk for long, and especially to walk fast, and for this he had been sent home. He was able to walk slowly a fair distance, but, if hurried, a painful stiffness came on in his legs which compelled him to stop. He said that there was never any real cramp, but a painful feeling of aching weariness in the muscles which

made it impossible to walk any further. It was, he said, "as if his legs were bandaged too tight." Sometimes this feeling would come on from merely moving about in his room, especially if engaged in any stooping occupation. His feet were always cold, both subjectively and to the touch. He felt them to be cold, but said that he had no wish to warm them at the fire. It was not a painful coldness. To my hand both his feet felt quite cold, and they were a little dusky near the ends of the toes, but there was nothing in the least suggestive of threatened gangrene. They were rather thin, but their state of nutrition was fair. The muscles of the right limb, both in the thigh and leg, were definitely larger than those of the other, but this he explained by saying that the larger limb had recently been very perseveringly massaged.

Mr. B—— brought with him several certificates and letters stating that he was suffering from sciatica. He had consulted several specialists.

The most detailed of his certificates, one written nearly eighteen months ago in India, stated: "The left femoral artery can scarcely be felt, and the circulation in both legs is excessively feeble."

The statement just quoted led me to make a careful examination of his arteries. I found his hands warm and his wrist-pulse full and of good power, being decidedly rather larger and firmer than natural, but without any indications of calcification of the vessel. In neither femoral, however, could I find any pulsation whatever, nor could the trunk of the vessel be easily recognised. Neither in the popliteal space nor at the ankles could any artery be found. I next sought for enlarged collateral vessels, both about the hips and over the knees, but neither could any of these be detected. My son also made a careful examination with a like negative result. Nor could either of us feel the external iliacs with certainty. Over the whole of the right limb a few varicose veins were seen, but there were none on the other side.

Having thus arrived at the conclusion that the supply of blood to the lower extremities was exceedingly defective, it

became of interest to ask for details as to the previous course of the symptoms. It occurred to me that possibly from beginning to end the symptoms which had been considered to be due to the nervous system were really to be explained by defective blood-irrigation. The facts which I subsequently ascertained seemed to support this view. The symptoms had never really been those of sciatica or of spinal disease. The patient was clear in his statement that in early life he had had warm feet, and that it was only about four years ago that he began to find his left limb cold and liable to ache. He recollected clearly the occurrence of the earliest symptoms in the other limb. This was about three years ago. He was standing about, out of doors, when suddenly he felt a numb sensation in the limb and weakness, as if it would give way. His thought was, "My good leg is going to fail me as the other has done." The symptoms partially passed off, but returned after a time, and after several temporary remissions, during some months, the limb finally settled into much the same condition as the first. From this date he was quite unable to walk far or fast, and was liable, when he attempted it, to aching pain in the legs. He also had much aching about the hips, but there was never any severe pain down the back of the thigh, like that of sciatica. The detailed Report from which I have already quoted (which was dated June the 23rd, 1874), spoke of "severe lancinating pains in the lumbar spine radiating round the abdomen and down both thighs and legs," and recorded that the "patellar reflexes were exaggerated, but that the patient could stand steady with the eyes closed, heels together, and head thrown back." Its writer was inclined to diagnose "some specific spinal lesion causing a disturbance in the neuro-muscular dynamics of the vessels of the lower extremities."

On a subsequent occasion I made another careful examination of the limbs with results in all respects corresponding with those recorded above. I could not with certainty even detect the abdominal aorta. I again inquired as to the dates and earliest symptoms. Mr. W—— told me that it was in May, 1892, whilst standing on parade, that he first had what he called "cramp" in the right lower extremity. By

"cramp" he meant severe aching pain. This recurred frequently for some months, and then ceased to trouble him. From this date the foot was chilly. In the following December again, whilst standing on parade, his other limb suddenly became painful, and felt weak. It also recovered to a large extent, but a year later both again failed, and from that time he had been disabled from riding and walking. He described the pain which followed exercise as being very severe, so that the perspiration would roll off his face. He never had any pain when at rest. The pain was usually an intense ache felt in the calves, thighs, and buttocks. It was wholly relieved by rest.

CASE IV.—*Tertiary Syphilis—Fusiform Gumma surrounding the femoral artery—Recovery under iodides—Obliteration of the vessel—Subsequently defects of nutrition in the foot.*

In the case of Mr. T—— we had interesting evidence as to the influence of defective circulation on the nutrition of the foot. Mr. T—— came to me in March, 1895, on account of some patches of dry eczema on his right foot. I did not at first remember who he was, but, observing that the foot was more dusky than the other, and that all the nails were thickened and broken, whilst those in the other foot were in a healthy condition, it came into my mind that I had attended him many years before for a fusiform gumma around the femoral artery. I asked him whether he had not once had a tumour in the thigh. He replied, "Yes, but that has quite gone away." On examination I found that there was no pulsation in any part of the femoral artery, nor could I any longer detect any trace of the aneurismal dilatation. Pulsation could be felt feebly on the brim of the pelvis, but not lower. Thus it was clear that the defects of nutrition in the foot were consequent upon defective arterial circulation. These defects consisted, as has been already said, in a thickened and broken state of the toe-nails and in a condition of pallor with slight lividity of the whole foot. There were also some patches of papillary eczema of indefinite character.

Mr. T—— said that his foot often felt a little numb, and that he was not able to stand on it long with comfort. Thus, when standing he always rested his knee on a chair, and had produced a patch of thickened and roughened skin by doing so. He was able, however, to walk almost as usual, and during the six years which had elapsed since I had treated him for the arterial dilatation he had had no particular ailment. I ought, perhaps, to speak rather of a fusiform gumma around the artery than of any real aneurism. Through a considerable length of its course the vessel was very much thickened, and although at first there was abnormal pulsation, I became aware, as the case went on, that the artery was undergoing obliteration, and towards the end of the treatment all pulsation in it had ceased. The treatment consisted in the administration of full doses of iodide of potassium. Mr. T—— had suffered from syphilis in his early life. The case is a good example of tertiary syphilitic disease of the arterial coats, and as such is of much interest as exemplifying what may possibly occur in connection with the vessels of the brain. The patient was a man of middle age. He was married and had a healthy family. Only this one artery, so far as we knew, had ever been affected, and in it a long, fusiform gumma was developed around the vessel, which led to its complete obliteration.

In this case it will be seen that the obliteration was of the superficial femoral in Hunter's canal, and that the profunda was probably still free. This explains the less severe disturbance of nutrition and of function as compared with the previous cases. I have placed the case in association with them, because it not only shows similar acroteric defects persisting for years after the obliteration of the artery, but because it also demonstrates one of the modes by which such obliteration may be brought about. When Mr. T—— consulted me for his thigh he had a fusiform swelling four to six inches long, and as thick as a large ruler, in the middle of the thigh. It was easily felt, and although tolerably firm, pulsated distinctly. I wished Mr. T—— to keep at rest, but this he declined, and he continued to visit me for some months, still following his usual avocation. I found the

swelling gradually diminishing under the iodide, and its pulsation becoming more and more feeble. I had attended Mr. T—— for tertiary symptoms previously, and I do not think there could be any doubt that it was a peri-arterial gumma which was attended by general dilation of the vessel itself. That obliteration should follow was a very interesting and somewhat unexpected occurrence.

CASE V.—*Obliteration of the left carotid artery with right hemiplegia — Absence of pulse in the right brachial and femoral arteries—Obstruction of iliac veins.*

I have now to add one of the most remarkable cases that I have ever seen. A gentleman, aged about forty-six, was brought to me with the statement that he had large tortuous veins in his abdomen and left thigh. He was partially hemiplegic in consequence of a fit three or four years ago, but about that I was not consulted. He could walk well, notwithstanding his weak leg, and had the appearance of robust health. He undressed in order that I might examine his enlarged veins, and I then found that the superficial epigastric on the right side was very large and tortuous, and it was easy to prove by compressing its trunk that its current was reversed. There were also some big veins coursing over his thigh and hip. The condition pointed to the probability of an obstructed iliac vein on this side.

Almost as a matter of routine I proceeded to examine his groins, and then found that his femoral artery was feeble in the left (non-paralysed) side, but that it could not be detected at all in the other. I next examined his other arteries, and found to my astonishment that he had no pulse in the right radial or brachial and none in the left common carotid. It will be seen that the arteries in both upper and lower limbs on the paralysed side were occluded. That this was not the cause of their weakness was, however, quite clear, for the occlusion of the opposite carotid fully explained that and also the occurrence of the fit.

It now became of interest to ask as to the nature of the fit and the symptoms which followed it. I was told that

our patient, a fox-hunter, had done a hard day's riding and was in perfect health the day before the fit occurred. His friends, however, thought in the evening that he looked as if he had taken a chill. Next morning, after breakfast, he rose to stir the fire, and fell forward insensible. He was found by his servant quite unconscious on the hearthrug. After this he was in bed some weeks, during the early part of which time he was unconscious and afterwards hemiplegic and aphasic. From this state he gradually recovered, and had regained the full use of his intellect, but with still a very definite tendency to rely upon certain set phrases, which he often repeated. There had been during the illness but little trouble with the sphincters, but they still, he told us, remained somewhat untrustworthy. As explanatory of the very severe cerebral disturbance which followed what we may presume to have been sudden occlusion of the left common carotid, it may be suggested as not improbable that the vertebral on the other side was previously obliterated.

There were no special lesions of nutrition in the right limbs, which it will be seen were both paralysed and deprived of blood supply. As to the date of the occlusion of their arteries there was really no evidence whatever. Gangrene had never at any time been threatened, nor had there ever been any special pain in them. They were always cold and liable to ache when used. They were somewhat wasted, and the fingers of the hand were contracted into the palm. The patient made very light of his ailments and complained of nothing. He denied that he felt the paralysed limbs colder than the others. The knee reflex was excessive. The pupils were normal and acted fairly well. No facial paralysis was obvious, but he could not whistle well.

It was difficult to find any probable cause for such extensive obliteration of large arteries. There was a possibility that the patient had many years ago had syphilis, but the evidence was almost nothing. It was certain that he had passed through a severe attack of typhoid fever some years before his fit.

The obstruction of the veins had attracted attention before other symptoms. On one occasion, when out shooting, he

had complained to a surgeon present that he could not walk any further and was in pain. The surgeon on examining him found his veins in the left hip enormously distended, as also a varicocele from which he suffered. He had been an athletic man up to the date of the fit.

Such cases as this suggest that it might be of interest and advantage to examine the arterial system more often and more systematically than is usually done. This patient had been attended after his fit by a distinguished specialist, but unfortunately the finger had never been put on his carotid, or the cause of the hemiplegia would have been at once obvious. It may be suspected that obliteration of large arteries is more common than is supposed, for not only do these vessels, as a rule, escape notice during life, but they are seldom or never examined post-mortem.

We must await a much larger collection of facts than we at present possess before we can form any trustworthy opinions as to the causes of these obliterations. In the present instance the patient was of gouty stock, but he had never had rheumatic fever, and I was assured by a physician that his heart was sound. To judge from my own observation, the sounds were distant and muffled, but I did not make a satisfactory examination.

It is remarkable how little detailed information has been recorded by surgeons as to the subsequent condition of limbs after the ligature of large trunks such as the common carotid and the iliac. Temporary hemiplegia has been often observed after tying the carotid, but I do not know of any case in which the loss of consciousness was so complete as in the present case, or the degree of persisting hemiplegia so great. Everything in the case points to the probability that the lesion (thrombosis) in each instance was local and involved a large vessel. The restoration of collateral circulation appeared to have been easily attained.

It will be seen that the patient has lost three of his main arteries (the left carotid, right subclavian and right femoral), whilst he retains only three (his right carotid, left subclavian and left femoral).

AN EXTRAORDINARY CASE OF MORPHŒA HERPETIFORMIS.

I WAS indebted to Dr. Mann, of Ashton-under-Lyne, for the opportunity for investigating by far the most extensive example of herpetiform morphœa which I have ever seen. The eruption covered, indeed, almost the whole of the patient's body and limbs, yet it presented the most definite indications of location by nerves and thus differed altogether from the diffuse and acroteric forms.

Herpetiform Morphœa so extensive that almost the whole surface of body and limbs was involved—Escape of the digits, palms, and soles, and of the fifth nerve territories—Description of the condition in the seventh year of the disease.

The subject of the case was a thin emaciated man who had suffered from the disease for about seven years. During the whole of this time he had been crippled by the contraction of his knees, and for the most part confined to bed. Six years ago he had been an inmate of the Manchester Infirmary under the care of Dr. Leech, by whom the disease had been first diagnosed. This was about a year after its commencement, and marked the date of absolute arrest of spreading. Since then the man assured me there had been no extension anywhere. To use his own expression, "Dr. Leech said he would stop it, and he did." Possibly the arrest of its development had occurred a little before that date. It was difficult in the absence of any professional observation to obtain a detailed history of the early development, but the following as a general statement may, I think, be relied upon. Thirteen years ago a patch, "at first like a pimple and afterwards thick and brown," formed in

front of the left forearm. This patch remained solitary for seven years. It did not disable him from his work as a gas-smith, and he took but little notice of it. It was still present when, seven years ago, a general outbreak over the whole body and limbs occurred. No cause could be assigned for this, and although from the beginning the patches were very extensive and numerous, yet during the next six months, at least according to the man's belief, they continued to extend. His knees were almost from the first stiffened and crippled. At about the end of a year he came under Dr. Leech's care, and from this date there was no aggression. Of late years the affected districts of skin had been improving, having become less stiff and thinner. This improvement was possibly in part due to the treatment which had been carried out under Dr. Mann's direction, and which had consisted in the continuous use of the tartarised antimony in full doses. This drug had seemed to soften the skin and render it less irritable. During the period of outbreak the man reported that he was feverish and ill.

I feel it to be an almost hopeless task to attempt to describe the patient's condition so as to convey to the reader any clear idea of what it really was. I will first enumerate certain parts which had wholly escaped and upon which the skin was still thin, supple, and elastic. These were the whole scalp, fore forehead, nose, middle regions of cheeks and chin; the axillæ; a broad elliptical tract of skin in front of the elbow, the palm of the hand, and palmar and dorsal aspects of fingers; a broad belt of skin across the back above the loins; the soles of the feet and the toes, both in dorsal and palmar aspects. The changes, so far as I could observe, were bilaterally symmetrical. On most of the parts not mentioned as exempt, the skin was tight and presented either a mottled vascular surface like that of a thin scar infiltrated with tallow or a well characterised, dirty-white, ivory patch. The best ivory patches were on the face and the lower part of the abdomen, but everywhere these were [illegible]ently in an advanced stage of retrogression. In many [illegible]les the skin was so thin, tight, and vascular, that it [illegible]d as if on the point of giving way, and in several, notably

on the knees, ulcers had formed. On many parts the margin of limitation of the diseased parts was very abrupt, but on some it was not so. Thus on the dorsal aspect of the foot it advanced to the roots of the toes, and there ended definitely by a line which crossed them transversely at almost exactly the same level. Below this line the skin of the toes was quite soft and normal. On the backs of the hands there was no limitary line, the changes ending gradually about the knuckles. You might shake hands with the patient without perceiving any peculiarity in the skin of his hand. The changes became definite, however, just above the wrist and involved the whole extremity, back and front, with the exception of the front of elbow and the armpit. The sides of the neck were infiltrated, and the condition extended as a large white patch in front of the ear over the parotid and adjacent part of cheek. A most singular appearance was here presented, for there passed forwards, from the parotid patches, horizontal belts under each eye, of a finger's breadth, the ends of which almost reached the nose. These bands were of a bluish white, and gave him the appearance of having been painted or enamelled. They did not involve the whole eyelid, for a narrow tract of healthy skin separated their upper borders from the edges of the lids. Below, their margins were very abrupt, the skin of lower part of the cheek being quite supple. The ivory patches extended upwards on the temple almost into the scalp hair, and there ended abruptly. Behind the ears, on the ears themselves, and on adjacent parts, the skin was everywhere involved. The nose, the whole of the lips, the central parts of cheeks, and the chin were quite normal.

The limbs, more especially the legs, were very thin, but no paralysis of special muscles could be recognised. The knees had never been extended for years, and thus the wasting might, to a large extent, be explained by disuse.

This patient was brought before a meeting of the Neurological Society at my Clinical Museum on November 7, 1895. In attempting to interpret the distribution of the patches by nerves, I ventured to suggest that although the face was so

extensively affected, yet that the fifth nerve was not itself concerned. The parotid region supplied by the great auricular was clearly implicated, and the bands under the eyes and those on the temple might not improbably be located by twigs from that nerve.

The total absence of acroteric phenomena was a most noteworthy feature. The exemption of the digits clearly went with that of the palmar and plantar surfaces, and it was somewhat difficult to explain as due to any special nerve trunks, since it comprised all the digits alike. The considerable tracts of skin in the axillæ and on fronts of elbows which were normal, were also not easily assigned to any special nerves.

It remains to notice the part where, according to the man's statement, the first patch had been located. It showed no great difference from the rest of the affected regions, there being only a little more thickening than in other parts. It looked like a thick ivory patch in the stage of retrogression. The possibility occurred to my mind that this patch might have been one of keloid of scar and not of morphœa. In the case of the woman Nichols, published by Dr. Addison and subsequently in the New Sydenham Society's Atlas, keloid of scar, or something which was so diagnosed, preceded the morphœa outbreak and subsequently disappeared by absorption.

A FATAL CASE OF VARIOLOID VACCINIA.

For the opportunity of observing the following case I was indebted to Dr. Greenwood, the vaccination officer of the Marylebone district. Dr. Greenwood was also good enough to take much trouble in facilitating the temporary removal of the body to the Clinical Museum, where it was seen by many members of the profession.

The case in all respects is an almost exact counterpart of the one which I reported to the Medico-Chirurgical Society in 1881. In each instance a healthy child vaccinated with pure lymph, and in whom the vaccination pocks had progressed well, died with high fever and a generalised eruption which much resembled small-pox. In the former case the gangrenous condition of the eruption attracted more attention than it would have done in the present one. This, however, is probably to a considerable extent a question of stage, for many of the places were obviously on the way to gangrene, and had the child lived longer would probably have become definitely so. The term "vaccinia" may be appropriately used for the sum of the febrile phenomena which follow vaccination. There is, of course, always some fever. It may run high, and be attended by more or less abundant eruption. In rare instances the fever and the eruption may be so severe as to cause death.

Dr. Greenwood told me that he had vaccinated, he believed, thirty thousand children, and had never before had any mishap. He had vaccinated this child himself, and had inspected it himself on the eighth day. On this latter date nothing unusual was noted, the pocks were perfect, and there was no undue inflammation. A few days after the inspection the infant appeared ill, and its mother consulted

the surgeon who usually attended her family. He found the child to have an eruption, which on the next day was so like an early stage of small-pox that he felt in much doubt as to the diagnosis. As none of the spots were definitely umbilicated, it was decided that it was not variola. Neither at this date nor for some days later did the infant appear to be seriously ill. The facts are summarised in the following schedule.

Death from Varioloid Vaccinia on the nineteenth day.

An infant, male, æt. 8 months.

DAY.	DATE.		DETAILS.
1	Nov. 27	Wednesday	Vaccinated from arm to arm.
2	28	Thursday	
3	29	Friday	
4	30	Saturday	
5	Dec. 1	Sunday	
6	2	Monday	
7	3	Tuesday	
8	4	Wednesday	Inspected. All well, five good pocks.
9	5	Thursday	Began to be feverish.
10	6	Friday	Eruption observed on the arm and on the face.
11	7	Saturday	Eruption increased and becoming general.
12	8	Sunday	A surgeon consulted.
13	9	Monday	Doubt felt as to whether it was small-pox.
14	10	Tuesday	Taken to St. Mary's Hospital.
15	11	Wednesday	Worse. Diarrhœa.
16	12	Thursday	Seen by Dr. Chilcott. Temperature 103.
17	13	Friday	Temperature rather lower. A fresh crop of eruption. Renewed doubt as to small-pox.
18	14	Saturday	Very ill. Temperature 104·5.
19	15	Sunday	The child died early in the morning.

From first to last the vaccination vesicles had progressed well. There had been no undue inflammation round them, nor any satellites.

I saw the child with Dr. Greenwood the day of its death. The body was well nourished, and that of a fine child. The following particulars are based upon an inspection of the body about six hours after death.

There were five pocks, each covered by a dry, blackish scab, on detaching which a thin, newly-formed scar was disclosed. There was no swelling, nor any trace of discoloration near to the pocks, the skin being quite white.

The trunk and limbs were covered with eruption, of which two kinds were to be noticed. One consisted of dryish crusts and scabs, varying in size from a split pea to a fourpenny bit. Some of these were raised in the centre, whilst others looked as if before drying up they had been depressed. These crusts occurred scattered singly over all parts. They might easily have been taken for variola in the stage of drying up.

The second type of eruption was seen in greatest perfection on the forearms, but occurred also on the trunk and lower limbs. It consisted of red papules, which were numerously confluent, covering areas of considerable size. These papules each showed a distinct depression in their centres. None of them had vesicated. They had the appearance of having been quite recently developed, and I was told that they had appeared only two or three days before the child died. This eruption had the appearance of an early stage of confluent small-pox.

The penis and scrotum were ecchymosed, and looked as if about to slough. Over a large area on the sacrum and buttocks the epidermis was loosened, and the skin beneath was discoloured and evidently in an early stage of superficial gangrene (just like the portrait which Sir W. Stokes gave me). Some of the patches on the abdomen were also in an early stage of gangrene. The subject of this case was a healthy child of eight months. Its parents were in good health, and had two elder children also quite well. Both of these had been vaccinated without any mishap. All three children had passed through chicken-pox within the last six months.

I reproduce in schedule-form the principal facts of the case to which I have referred as published in the Medico-Chirurgical Transactions.* It is of interest to note how closely it resembles the case which I now record. One died on the nineteenth day, the other on the twentieth. In one the eruption began on the seventh day, in the other on the ninth.

* I possess a few reprints of this paper, and shall be glad to send a copy to any one interested in the subject.

Death from Varioloid Vaccinia on the twentieth day.

An infant, male, æt. 3 months.

DAY OF VACCINATION.	DATE.	DETAILS.
1	Nov. 11	Vaccinated from arm to arm.
2	12	
3	13	
4	14	
5	15	
6	16	
7	17	An eruption observed by the mother.
8	18	Inspected. Four good vesicles; an eruption in early stage.
9	19	The eruption was "shotty," and was supposed to be variola.
10	20	Eruption increasing.
11	21	
12	22	Some of the pustules had become gangrenous.
13	23	
14	24	
15	25	
16	26	Not seen by any medical man. The body and limbs covered by an eruption, of which many of the spots looked like variola, and became crusted, but many others passed into gangrene.
17	27	
18	28	
19	29	
20	30	The infant died.

The Clinical Museum possesses portraits from two other similar cases, in both of which, however, the patient recovered. One of these was given me by Sir William Stokes of Dublin; the other by my late son, Procter S. H. I do not know the exact dates in either of them.

These cases, lamentable as they are in themselves, are of the utmost value in reference to the theory of vaccination.

They go far to prove what some of late have been inclined to dispute, that the vaccine virus now in use is really derived from variola. They show that it is capable of producing a severe fever, distinguishable from small-pox only by its shorter stage of incubation and by the absence of any infective power. In none of the cases which I have quoted had there been any known exposure to the risk of contracting small-pox, and in no one did any other cases develop in the house. On these points careful inquiries were made. In each of the fatal cases the dead body of the infant was taken to a Medical Society and seen by many. Allowing for the probability that most were well protected by vaccination, it is yet extremely improbable that, had the disease been an infectious one, we should not, either in the child's family or amongst those who saw the body after death, have had any single instance of its spreading. The cases give support to each other, and there can be but little doubt they are examples of vaccinia in its most severe form. To them the adjective varioloid seems applicable.

It would seem probable that the eruption of vaccinia may in different individuals assume very different forms. In most cases it is a slight affair, an erythematous or lichenoid eruption, beginning from the seventh to the ninth day, and not lasting long. About this class of cases, however, we need more detailed information. They seldom attract much attention. Another class of cases, probably very rare, is marked by the formation of a scanty eruption, but of formidable aspect, in which scattered vesicles develop to a large size, become umbilicated in the most characteristic manner, and exactly resemble those of gigantic variola. I have seen but two or three of these. They were indistinguishable from variola, but I believe there was no contagion, nor, although the children were seriously ill, did any of them end fatally. Our faith in the protective power of vaccination ought surely to be strengthened by the occurrence of such cases as these.*

It is clear that the conditions which thus permit, as it

* What I have written in Catechism at page 100 was in type before the case now recorded occurred.

were, the vaccine virus to reassume the powers of its parent, variola, reside not in the virus itself but in the recipient. In all the cases to which I have referred other children had been vaccinated with the same lymph, and nothing unusual had occurred. It is worth noting, further, that in all instances the vaccine vesicles themselves were progressing quite normally. What the conditions are which, present in the infant, may favour or permit an unusually severe development of vaccinia, it is impossible to conjecture. That it is not anything which can be classed as debility or ill-health is obvious, for all the children were remarkably healthy. In two instances the child was reported to have recently passed through an attack of chicken-pox, but I am not aware of any other facts which would favour the suspicion that this malady is likely to produce aggravated susceptibility.

ON CANCER AND THE CANCEROUS PROCESS.

(*Continued from Vol. VI., page* 88.)

Heredity of the tendency to Cancer.

THE estimation of the influence of heredity on cancer by recourse to what are called "statistics" is very fallacious. Many patients will conceal facts of which they are perfectly cognisant, and a yet far larger number know very little of what has happened in previous generations. Those who have attempted to collect statistics in reference to family tendencies to insanity know, perhaps, better than any others the proneness of relatives in all ranks of society to minimise, and even conceal or deny the facts. There is less temptation, and less tendency, to do this in the case of such maladies as tuberculosis and cancer, but somewhat similar motives do in less degree influence the mind even in reference to them. Every one is anxious to maintain the family honour, and freedom from disease of all kinds, in former generations, is accounted as more or less a matter of personal credit by most persons. If, therefore, the "statistics" collector is content to take and record the first answer that he gets to the question, "Have any of your relatives had cancer?" the result of his inquiries will, I believe, be worth far less than the paper and ink which he consumes. It is not to be denied that there is a source of fallacy in the opposite direction, and that in some cases ignorance may give a name to a disease which it did not really merit. This, however, is a far less important source of error than the other, and there can be little doubt that statistics on this subject must always, even when most carefully collected, understate the truth. Those familiar with cancer, as seen in practice, form, I have no doubt, much stronger impressions as to the importance of

[illegible] than would be obtained by consulting any of the [illegible]ical tables which have been compiled. The positive [illegible] which are forced on their attention unavoidably influence [illegible]ir minds far more than the array of negative evidence which statistics seem to supply.

Some years ago I was consulted in the same house for three near relatives who were the subjects of cancer, and I was told that there was a fourth, whom I did not see. In each individual the form of malignant disease was different. I have recently seen a patient who, after syphilis and in connection with smoking, has sores on his tongue, which have occasioned anxiety as to the precancerous stage. Inquiring as to his family history, it proved to be sufficiently alarming. He had lost a married sister at the early age of thirty-four, after three operations in one year for cancer of the breast, and one of his grandmothers and two of his aunts had died of the same disease. I have published the case of a young man who became the subject of rodent ulcer of the face at the early age of fourteen years, by much, I believe, the youngest age on record. Now the history of this patient was that his father had been the subject of the same malady some years before his son's birth, and had died of it twenty years later. In connection with this case it may be noted that when cancerous processes begin at an unusual early age there is almost always a history of inherited tendency. It does not, however, by any means always, as in this instance, manifest itself by the production of the same kind of cancer. More usually we encounter transmutation in transmission, and some different type of new growth is the result.

In connection with this subject, the case recorded by Mr. Ancell in the Medico-Chirurgical Transactions, and supported by two others which have been since published, are of great value. In these cases the common cystic tumour of the scalp had been hereditary in several generations, and had at length been developed in great multiplicity, and at an earlier age than usual. Finally, the disease assumed the form of solid tumours, which were reproduced on other parts than the scalp and proved malignant. In these cases we see, under the influence of heredity, an innocent form of growth first

exaggerated and made to occur earlier in life than it should, and next transmuted into one of undoubted malignancy. A young lady, at present under my observation, and for whom I have during the last five years excised a great number of sebaceous tumours of the scalp, is the daughter of a man whom I attended many years ago for rapidly growing sarcoma of one femur.

An example of the form of malignant ulcer of the face attended by thick growth, rather rapid development, and edges curled over outwards—No gland disease.

Mr. R——, who consulted me on July 20, 1894, presented a very curious form of malignant ulcer on his face. It reminded me much of Mr. ——'s case at Dunmow, in which enlargement of lymphatic glands has occurred. (See portrait.)

Mr. R—— is a florid, healthy-looking man of 75. His ulcer is a large elevated sore with overhanging edges (curled over outwards), and a perfectly clean, florid, weeping surface. Its surface is covered by clean granulations. It is almost round, and as large as a crown-piece, occupying the greater part of the left cheek. Nowhere is there any approach to the rolled edge of a rodent ulcer, nor is there the deep excavation and elevated borders of the crateriform. The base of the growth is extensive and very hard. By the finger inside the cheek it is felt as a rounded mass, well defined and close to the mucous membrane. It does not actually involve the latter. No definite gland enlargement can be detected.

Mr. R——'s history is that the ulcer began about three years ago, and that after it had been present a year, and when about the size of a shilling, it was excised. Six months after the excision there were signs of return, so that the present growth is of about a year. He was told by a distinguished surgeon six months ago that he had better let it alone, and that it would never shorten his life. On the latter point, however, there is not much question, for it is growing rapidly, and from its appearance, there is, I think, much fear that it will cause gland infection. This, of course, is the all-important question. Guided by what has happened in the

Dunmow case, I have strongly advised Mr. R—— to have the growth excised, and should purpose to take the whole thickness of the cheek, for it is too close to the mucous membrane to allow of the latter being saved.

It is to be observed that Mr. R—— has thin, florid, clear skin (like General K——), and that on many parts of his face there are little scaly patches of a threatening nature.

December, 1894. It is still a clean, florid ulcer involving the lower third of the cheek. It is prominent and has overhanging everted edges. It includes the whole thickness of the cheek and adheres to the upper jaw. During the four months since I saw him it has scarcely increased. A very remarkable feature is its absolute painlessness.

There is a deep crater in the middle, but the borders are flattened out.

Xanthelasma Palpebrarum in an old woman who had suffered from headaches in youth—Subsequent Jaundice, with Cancer of Liver—Cancerous Nodule at Navel.

A woman, aged 80, had characteristic patches of xanthelasma above and below the inner canthus of both eyes. She was also deeply jaundiced, the tint of her skin tending to black. At her navel there was a little patch about the size of a shilling, of scirrhus hardness. Her abdomen was distended in part by flatus and in part by ascitic fluid. The liver could be felt to be indurated and lumpy.

Comments.—When I first saw that this patient was deeply jaundiced, and that she had patches of xanthelasma on her eyelids, I expected to find patches of the latter malady upon her hands, the flexures of her joints, &c. None such were, however, discoverable, and my conclusion was that we must separate the xanthelasma from the jaundice, and regard it merely as one of the ordinary forms so frequently seen in those who have suffered from sick headaches. The jaundice was recent, and, I have no doubt, obstructive; but the xanthelasma has probably been present for many years. I have no doubt that she is the subject of malignant disease of the liver. The little nodule of scirrhus at her umbilicus is a very

important item of evidence in this direction, for cancer of the navel is seldom primary, and usually consequent upon infection which has travelled from the liver along the lymphatics of the round ligament. The only connection, therefore, between the xanthelasma and the jaundice, is that they both depend upon liver disturbance. The one, however, is due to often repeated functional disorder long ago, and the other to the recent development of cancer. It is a case in which jaundice has been superadded to xanthelasma, and not of the reverse.

POSTSCRIPT.—I was indebted to Mr. Hopkins, medical officer of the Cleveland Street Sick Asylum, for the opportunity of seeing this interesting case. The patient died about a month after my notes were taken, but, unfortunately, no post-mortem was obtained.

MISCELLANEOUS.

No. CXCV.—*Additional cases of Muscular Displacement of the Digital Phalanges.*

Dr. Donaldson, of Londonderry, has been kind enough to send me the following description of a case illustrating the liability to spontaneous displacement of a digital phalanx, which I described in the last issue of Archives. He writes that he is inclined to refer the liability to senile or rheumatic changes. It will be seen that the joint affected was not the same as that in the case which I described:—

"On October 7, 1894, a woman, æt. about 65, drew my attention to a peculiar condition occasionally assumed by the ring finger of right hand. In order to show me what occurs, she got a loaf of bread and commenced cutting a slice off it. In a short time she said, 'Now it's down.' On examining, I found the ring finger had become flexed, nearly touching the palm. She had no power to extend it without using the other hand. The joint between the first and second phalanges was fixed. She overcame the difficulty by extending the finger (or 'lifting it') with the other hand. A slight snap was felt at the moment of extension, and the sensation was a little unpleasant.

"She first noticed the peculiarity three years ago, when peeling an orange. During the last few weeks it had occurred more frequently than formerly. When closing and opening the hand without grasping anything the ring finger never remains flexed, but may be extended to the full extent without any difficulty. To produce the condition she says she must grasp something firmly, although it may occur even when she is combing her hair. The other fingers present no abnormality."

I will briefly mention the particulars of another case which I have recently seen.

My friend Dr. Hemming, of Kimbolton, has sent to me for examination an old gentleman who has been much annoyed by liability to this displacement. In him, as in Dr. Donaldson's case, the first phalangeal joint is the one affected, and not the metacarpo-phalangeal, as in my own. The subject of Dr. Hemming's case is an athletic man of seventy-four who has done much rowing. He has lived well, and is undoubtedly gouty. He has been annoyed by the liability to displacement for more than a year, and is afraid there is now commencing some tendency to it in the same finger of the other hand. He has derived no benefit from frictions, etc.

It will be observed that both these cases confirm the impression that this liability is in some way connected with rheumatism and gout. It appears also to occur chiefly in senile periods, although in the case which I first described the patient (distinctly gouty) was only fifty-seven. Younger persons occasionally complain that their finger joints are liable to slip and become stiff, but I have never been told by them of such an amount of dislocation as needed the assistance of the other hand for its reduction. It may be that those who have congenitally loose ligaments are those who in later life, when rheumatic changes have supervened, are liable to these more definite forms of displacement.

No. CXCVI.—*Spasmodic Stricture at Cardiac end of Œsophagus.*

It seems highly probable that there exists some muscular structure at the cardiac end of the œsophagus which renders spasmodic closure possible. Patients sometimes describe to us attacks during which it is quite impossible to swallow, the sense of obstruction being felt to be low down. Yet they can at other times swallow well, and do not lose flesh or present other signs of organic disease.

An elderly surgeon recently described to me such attacks, which he attributed to eating fish. He said that he could eat anything else well, but that he had often, when eating

white fish, been seized at table by severe pain at the epigastrium, and sense of absolute inability to swallow anything more, either solid or fluid. He had often been obliged to leave the table on account of the pain caused. No sickness ever attended the attacks. He had found ether give almost immediate relief.

No. CXCVII.—*Gout and Gonorrhœal Rheumatism.*

The association between inherited gout and gonorrhœal rheumatism is one respecting which I have long entertained no doubt whatever. Of late years I have never seen gonorrhœal rheumatism without obtaining the history of gout in the family. In the case of Mr. T. B——, aged 28, who came under my care for gonorrhœal rheumatism in March, 1895, I was acquainted beforehand with the family history. I had in former years attended his father, his mother, and several of his brothers and sisters: thus I knew that there was a strong history of gout in previous generations, and a tendency to rheumatic affections in the present one. This was the second time that Mr. T. B—— himself had suffered from gonorrhœal rheumatism. Three years ago he had been confined to bed three weeks with a sharp attack. He had had gonorrhœa several times. On the present occasion a slight discharge had made its appearance two days after intercourse. There was no pain whatever until injections were used. Subsequently he had severe pain and much discharge. He had been laid up two months at home before I saw him with gonorrhœal rheumatic ophthalmia and effusion into his left knee. Other joints had been affected, and he had suffered from very severe pain in his back. It may be added that at the age of 13 he had passed through a very severe and almost fatal attack of rheumatic fever, attended by delirium and implication of the heart. His heart is at present much hypertrophied, and there is a loud to-and-fro murmur. No case could be more definite as to the association between the constitutional predisposition and the exciting cause than this. Mr. T—— was himself a florid, well-developed, and very vigorous man.

No. CXCVIII.—*Chancres on the Lip supposed to have been contracted at a barber's shop.*

I have recently seen a gentleman with three indurated chancres in a row, on his lower lip. They were on the skin below the prolabium. All were quite characteristic, and they were attended by a number of enlarged glands under the angles of the jaw, quite loose and almost as hard as bullets. There was also a commencing eruption. The chief point of interest was, as to how the sores had been contracted. Clearly they were not "kissing-sores." The patient was inclined to think he had been infected at a barber's shop, for he remembered that he was touched by the razor in exactly the position which the sores occupied. It was, indeed, in just the part most liable to be cut, that is under the projection of the lip. It seems, however, very improbable that the edge of a razor, covered as it would be with lather, could convey syphilitic virus, and even if it did, that it should not be taken off in the subsequent washing. The more likely hypothesis is that the inoculation was effected by the towel last used to dry the part which the razor had abraded. On my suggesting this, my patient at once admitted its probability, saying that he had been shaved in a crowded room at a regatta, under circumstances which made it not at all improbable that the barber might have used the same towel for more than one of his clients.

No. CXCIX.—*Another example of Curving of the Penis.*

I have recently seen in consultation with Dr. L——, of H——, another example of the curving of the penis which was the subject of a report in ARCHIVES, Vol. V. p. 329. The patient was a married man in robust health, who was supposed to be "gouty," but without any conclusive evidence. He had passed through two attacks of cystitis, for which no cause had been discovered. The curving of the penis had troubled him for about a year, and had been such as to entirely prevent sexual congress. On drawing the penis

forwards an indurated band could be distinctly felt in the middle of the dorsum, near its root. At this part there was some loss of substance. The patient had no evidence of contraction of the palmar fascia. He had been advised to have the band dissected out, but having regard to the evidence of atrophic shrinking, already present, and to the probable effects of the scar tissue, I felt unable to concur in this recommendation, and could only suggest that the inconvenience should be borne with patience.

No. CC.—*Syphilis without a Chancre.*

I have seen several cases in which the most careful search failed to discover any primary sore, yet in which definite constitutional symptoms followed. The following may stand as an example. A young gentleman of 22 was sent to me by his surgeon on January 11, 1892. He had a papular and erythematous eruption, which I should at once have held to be characteristic had there been a chancre. But nothing of the kind was present. He admitted exposure to risk two months ago, but said that nothing whatever had been noticed on the genitals. I abstained from specific treatment until his second visit, on February 2, when he was covered with an eruption about the nature of which no doubt could be felt. I again sought for a chancre, but could find no trace, nor was there any definite enlargement of the inguinal glands. Mercury was ordered at this date, and I saw no more of the patient. In 1895 he came to me to know whether he might marry. He had taken mercury for 18 months, and had since been two years without it. He appeared to be in perfect health. He told me that he had never had anything except the eruption, and that it had disappeared completely in a month or two under the treatment.

No. CCI.—*On the Simulation of Lymphadenoma in Syphilis.*

In the first number of my ARCHIVES, I referred to a case then recently published by Dr. McCall Anderson, in which

some very large glandular masses resembling lymphadenoma had disappeared rapidly and completely under the influence of iodide of potassium. The mode of cure was believed to prove that the enlarged glands were really caused by syphilis. The other evidence in support of this diagnosis was but slight. Having regard to the extreme rarity of gland tumours of the kind described in connection with syphilis, I ventured to express some doubt as to whether the iodide cure really amounted to a proof of the specific nature of the disease. A case in which I have recently been concerned has presented a precisely similar problem. A man now of middle age was treated for syphilis in 1883. Having been told by Mr. Henry Lee that he was quite well and might marry, he took a wife three years later, and subsequently during six years enjoyed excellent health and was the father of three children, all of whom are living and now in excellent health. Nine years after his syphilis, however, he became the subject of lymphadenoma. Large masses of firm glands without any tendency to suppuration were developed in both sides of his neck and in both axillæ. After having resisted tonic treatment they yielded very definitely to iodide of potassium. After a six months' course of the latter, those in the neck had almost wholly disappeared. At the present date, three years later, little or nothing can be detected in the neck, but in both arm-pits there are still very definite bunches of enlarged and rather flabby glands. These are quite movable, and show no tendency to inflame. No other symptom of tertiary syphilis has occurred, but recently the patient has had a single attack of tongue-biting epilepsy and many of petit mal. Dr. Hughlings Jackson, whose assistance I have obtained, expresses doubt as to whether these fits have anything to do with syphilis, averring that he cannot find any feature in which they differ from ordinary epilepsy. It becomes, therefore, of extreme importance to know whether the enlargement of the glands, together with the mode of cure, affords any evidence of tertiary syphilis. If it does it certainly is amongst the very rarest of the phenomena which we observe in such connection. With the exception of Dr. McCall Anderson's, I do

not know of any well recorded cases in which lymphadenoma was simulated by syphilis.

No. CCII.—*Two cases illustrating Prognosis in Arterial Hemiplegia after Syphilis.*

Two valuable cases as bearing upon prognosis in hemiplegia after syphilis are those of Mr. H—— and Mr. W——. In both of these an attack of hemiplegia occurred early. Both recovered, and are now living. I saw both patients myself, Mr. H—— during his hemiplegia, and Mr. W—— when he was just recovering. In H——'s case it is now twenty years since the attack, and W——'s exactly ten. Both are married and have healthy children, but both are now the subjects of tabes in an incomplete form. In both the recovery of the paralysed limbs has been almost complete, and in neither has there ever been any tendency to recurrence of cerebral symptoms. In both the knee-jerk is excessive in the limb which was formerly paralysed, and in both the affected arm and leg are recognised as still being a little weaker than their fellows.

No. CCIII.—*Inherited Syphilis—Ringworm Tongue at the age of five.*

One of the most important facts which I have obtained as to the cause of the ringworm tongue, and its association with inherited syphilis, is detailed in the following narrative, which includes also some other interesting details:—

On June 20, 1895, a Mrs. S——, whom I had seen five years previously both on her own account and that of her child, then an infant of a year old, brought me the latter in order that I might see her tongue. It was in the most characteristically ringworm condition that I have ever seen, being covered over its dorsum and sides with large, abruptly margined patches. Mrs. S——, a most intelligent lady, assured me that these patches were extremely fugacious, and when I suggested that I should like a sketch done in the course of a few days, replied that they would probably be

much altered by that time. She said that the patches came and went, and were never two days alike, and she believed that they were under the influence of mercury, and that she could at any time cause their disappearance by rubbing in the ointment. Having been made acquainted with the nature of the child's illness, and having read works on syphilis, she had been accustomed to treat both herself and her child very freely by mercurial inunction. This had been going on for five years, during which time she had been resident in India. I referred back to my first notes of the case, and found that in 1891 I had seen husband and wife and child together, and had recognised inherited syphilis in the latter. The child was the firstborn of her mother's second marriage, and her father admitted having had syphilis within two years of the conception. Thus the fact that the child is syphilitic is beyond doubt, and it becomes of much interest to note that, with the exception of the ringworm tongue, it showed but few traces of the disease. It had not yet got any of its permanent set of teeth, but its first set was beautifully white and perfect. Snuffles had been its principal symptom in infancy, and the bridge of its nose was now very conspicuously sunken, but beyond this there was nothing in its physiognomy which presented any peculiarity. It had a good skin, and was on the whole a pretty child. In reference to the soundness of the first set of teeth, it is to be remembered that the mother had taken no mercury during her pregnancy. The disease in her did not manifest itself until some time after the child's birth.

No. CCIV.—*On the frequency of Phagedæna in Second Infections of Syphilis.*

The belief that when a primary syphilitic sore becomes phagedænic it is very often in consequence of the patient having suffered from syphilis on a former occasion, is one which I have long entertained and taught. Two remarkable instances in confirmation of it have recently come under my notice.

In the first I was asked to see a middle-aged man in con-

sultation who had had some weeks of vigorous treatment for phagedæna of the glans penis without complete result. He still had a sloughy sore, and a great part of the prepuce had been destroyed. My usual inquiry, "Has he had syphilis before?" was at first answered in the negative. He was a married man with healthy children, and apparently in robust health. He had admitted to his medical attendant that in youth he had had gonorrhœa, but he had denied syphilis. On coming to our patient's bedside I pushed my inquiries, and then found that there was doubt. There had been once a soft sore, and there had since been ulcers in one leg. On inspecting the leg, large deep scars with pigmented edges were seen which bore conclusive testimony to former disease.

Capt. G—— came to me with hard sores which were swollen, inflamed, and at parts phagedænic. There were three of them, and they had probably been contracted a month before I saw them. The history was that Capt. G—— had eighteen years ago had a sore which was attended by a bubo which did not break, and followed by an eruption. Fourteen years later he had another sore which was much inflamed and for which mercury was given, but which was not followed by any other symptoms.

No. CCV.—*Inherited Syphilis without defects in Physiognomy—Very severe Keratitis relieved by Setons—Vitreous Opacities and Choroiditis—Synovitis of one Knee.*

Miss S——, who has attended at my Demonstrations, affords an interesting example of the order of events in inherited syphilis. She is now aged 12, stout, well grown, and of a blooming, florid complexion. She has a large scar at the left angle of her mouth, which was left by a sore in infancy. This is almost the only revealing symptom which she presents, for although her head is large and somewhat squared, it presents no definite bosses, and her teeth show nothing that can be called characteristic. They are of the mercurial type. Miss S—— is an only child. Although when in my

presence always so nervous that I suspected defective intellect, her father assures me that she is remarkably intelligent, and has always excelled other girls of her age. She has been remarkably proficient in music.

At the age of seven she had a very severe attack of keratitis, and ever since her sight has been defective. After she had had much treatment for her eyes she was brought to me, and I put setons in. These were apparently productive of immediate benefit, and her corneæ are now quite clear.

A few months ago Miss S—— was brought to me for effusion into one knee, and at this date (March, 1895) I took her to the Demonstrations. Under iodides she quite recovered, and her knee is now well.

To-day, August 13, 1895, I examined with the ophthalmoscope. Her pupils dilate well with atropine and are round. The vitreous in both shows films and dense black opacities, but the details of the fundus are easily inspected. There is very extensive choroido-retinal mischief in both, more especially near the periphery. Large and small masses of coal-black pigment, some spider shaped and others shapeless blotches. Also many patches of denudation abruptly margined, but for the most part small. The discs are slightly waxy, and their central vessels small. She still reads a great deal, but holds her book close.

No. CCVI.—*Syphilis and Marriage—An instance of severe Family Syphilis.*

A very interesting case bearing on syphilis and marriage is that of Mrs. P—— and her family. I have reverted to it repeatedly in illustration of one or other point in reference to the inheritance of syphilis. Mrs. P—— herself suffered from syphilis soon after her marriage, and in later life was the subject of tertiary disease of glands and of bone. She, however, retained good general health, and, knowing nothing as to the nature of her malady, she was always, as she still is, a cheerful and happy woman. Her husband, who was the cause of the whole, soon got rid of his symptoms, and remained, I believe, in excellent health. They have three

living children, and all have suffered very definitely. All have gone through attacks of interstitial keratitis. One has lost her soft palate, and one is absolutely deaf. The three are now between the ages of 15 and 21. In spite of what they have suffered they are all well grown and rather good-looking. No one would suspect any of them of being the subjects of hereditary taint. In all the eyes have perfectly recovered, and they can see as well as ever. The boy suffers no permanent disadvantage of any kind, and probably never will. One of the girls, having lost her soft palate, has a nasal voice, but this is her only permanent trouble. Her sister suffers from what must be acknowledged to be serious enough—permanent and absolute deafness. I asked her mother whether this deafness made the child unhappy. She assured me that, so far from it, this daughter was one of the happiest girls she knew. She had developed a great taste for painting, and found in it a constant resource. Her mother added that she had never known her to express any wish that she could hear.

I quote these facts, not from any wish to suggest that the miseries caused by family syphilis are more imaginary than real, but as one of the most definite examples of severe injury which has fallen under my notice, and in which yet the marriage cannot be regretted.

Had the parents been made acquainted with the real nature of their children's suffering, they would probably have been very unhappy. As it is, I feel sure that they are not so, and that not any one of their children feels otherwise than thankful for existence. I could quote hundreds of families in which far more real suffering and social disability has been caused by the inheritance of scrofula, or of neurotic tendencies.

No. CCVII.—*A Note on Hermaphrodites.*

So much of what is repulsive attaches to our ideas of the condition of an hermaphrodite that we experience a reluctance even to use the word. Yet it is very desirable that we should gain clear ideas on the subject, and in the pursuit of

sound knowledge nothing must disgust us. It lies at the very root of the matter to acknowledge that, like many other conditions, hermaphroditism is a thing of degree, and that up to a certain point all persons are bisexual. Up to a certain age the fœtus has potentially the organs of both sexes, and it is only by the ascendant development of the one set that the other is suppressed. Nor is the suppressal in either sex ever absolute, for every male has mammary glands and every female has a clitoris, organs which definitely belong to the other sex, and which persist only because the suppressal has been incomplete.

If we define the word in its old Greek acceptation as implying a being capable of self-fecundation, then it may be admitted that it is very improbable that amongst the higher mammalia any such will ever be found. But improbability based upon experience is all that we can assert, for we must remember that all that is necessary to that result is that sperm and germ organs, with their appropriate canals, should be present; and it has been proved by repeated dissections that testes, ovaries, and uterus may exist in the same individuals. Experience, however, shows that whenever this is the case, the organs are never developed in perfection. The testes do not secrete semen, or the ovaries do not attain their functional activity, and thus the being who is in a sense bisexed is in another sense unsexed, and never attains the full development of either.

We meet here only with an illustration of the general law of nature, that in order to the attainment of high excellence in one direction there is usually definite suppressal in another: when one faculty is greatly in the ascendant, others are usually as definitely abortive.

A very interesting question arises here as to how far the special organs of sex are inseparably connected with its moral and emotional characteristics. Roughly speaking, and in a general way, no doubt they are so, and the more perfect the organs of the one sex and the more nearly absolute the suppression of those of the other, the more marked will be the other sexual characteristics. Yet in connection with this we may suspect that it is the fact that sex may be defective

without any abnormality of the organs to be appreciated by the anatomist. This is a matter of common knowledge, and its investigation is chiefly of scientific interest in reference to the endeavour to ascertain whether in such instances organic peculiarities which have been overlooked do yet really exist.

No. CCVIII.—*An Illustration of the value of Inaugural Theses for Diploma Examinations.*

The worse than uselessness of accepting Inaugural Theses as proofs of scholarship is well illustrated by the Wright-Ludford transaction to which I have been obliged to refer at page 28. Dr. Wright, F.R.S., an eminent and most excellent man, saw nothing to be reprehended in his composing for his ward (who became Dr. Ludford) a thesis on Yaws, which the latter translated into Latin and presented and afterwards published as his own production. Dr. Wright's biographer also saw nothing wrong in the procedure, and having found the original draft of the Thesis in Dr. Wright's own handwriting, published it in full in English amongst the latter's collected works. It had already been quoted by Dr. Maxwell, and probably by others, as "Dr. Ludford's excellent Essay on Yaws." The thing has probably been often done, but probably not often acknowledged quite so freely. In this Essay, as now published in Dr. Wright's own name, the following occurs:—"Born in an island where this disorder is exceedingly prevalent, and deeply interested for the honour and welfare of my native country, I have," &c. This deceived me, and I naturally thought, and had indeed written, that Dr. Wright was a native of the West Indies, but, on referring to his biography, discovered that he was also born at Crieff, N.B. These words were in fact penned for his ward to transcribe, and the deception is only emphasised by another sentence in which the latter is made to acknowledge his "indebtedness to a valuable friend."

Such practices are not only immoral, but definitely inconvenient. Not only does Dr. Ludford obtain credit for work in which he had no share, but the essay published under his name loses in the authority which would have been accorded

it had it been known to be written by so distinguished a man as Dr. Wright. Further, there is great risk, now that the same essay has been given to the world under two names, that it may be quoted as expressing the conclusions of two separate observers. I had myself very nearly done this. Dr. Wright's biographer does not mention Ludford's name, and it was by accident that I discovered that the same thesis which I had been painfully perusing in bad Latin was ready to my hand in English under the name of another author.

No. CCIX.—*An Early Account of the Itch Insect.*

In Dr. Joseph Adams' work on Morbid Poisons (second edition, 1807) there is a curious chapter, of which the following is the author's synopsis :—

"*Chapter* XIX. *Account of the Acarus Syro (Exulcerans of Linnæus), by some considered as the Itch Insect. Controversy concerning its existence and source of this difference of opinion*, 293; *progress of these insects in the author and part of his family*, 295; *itch and the disease from the Acari district*, 299; *subsequent remarks on the mode of conducting this and many other controversies*, 301."

The author states that the first account of the insect, of which he knew, was by Dr. Mead, in the "Philosophical Transactions," based on a letter from Dr. Bonomo, of Rome. He records the fact that Hunter was incredulous as to its existence, asserting that he had never been able to find it. Dr. Adams and his friend Mr. Banger both tried successfully the experiment of "colonising the insect in ourselves." A very full and interesting account of what followed is given. He asserts that the insect "can leap with a force not less than a flea." An interval of three weeks occurred between the placing of the acarus between the fingers and the first irritation. The itching began simultaneously on different parts of the body without any obvious eruption. A fortnight later

there was general efflorescence, but not many vesicles. At this date a skilled old woman readily extracted two insects from Dr. Adams' arm. The upper and under surfaces of the insect are very correctly figured. In spite of these apparently conclusive experiments, Dr. Adams still argues that there is a difference between the disease commonly known as "itch," and the vesicular eruption due to the parasite. The fallacy is that he recognises under the latter only recent cases with fresh burrows, from which the insect is easily extracted, and relegates to the category of "true itch" the advanced cases in which, by scratching, a mixed and general eruption has been produced, and the vesicles and burrows concealed. The paper displays throughout the philosophic caution and zeal for science which distinguished all that its author wrote. It contains a summary of the early literature of an important discovery, and is well worthy of republication.

No. CCX.—*Herpetiform Streaks or Ribbons—Devergie's description of them.*

Devergie has described a form of lichen which he names "*perpendiculaire ou en ruban.*" The latter expression he treats as if synonymous with *gyratus*, and says that it is the very rare form first described by Biett. He quotes from Cazenave's edition of Biett the statement that the papules, disposed in little groups, form a sort of ribbon on the chest, which, starting from the front of the chest, passes down the internal part of the arm and along the internal border to the extremity of the little finger, following exactly the course of the ulnar nerve. Devergie gives an illustrative case of his own, apparently the only one that he had seen. A man of 37 showed a ribboned eruption of this kind which passed from the fold of the buttock along the inner and posterior part of the thigh over the popliteal space and down the posterior aspect of the leg, losing itself upon the outer border of the foot. The ribbon was very narrow, and was continuous throughout its long course. It had commenced ten months before, and had been preceded by some discomfort at the anus, which was followed by some

little papules at the inner and upper part of the thigh which soon extended down the whole length of the limb. The man remained three months in the hospital, took tincture of cantharides, and used various ointments and caustics. It is implied that he was cured by the nitrate of silver. ("Traité pratique des maladies de la peau," by Professor Devergie, third edition.)

No. CCXI.—*Biett's Bands.*

The Clinical Museum contains some excellent illustrations of the ribbon-like streaks described in the above note.* Some of them were executed many years ago. I have long taken great interest in the subject, but was not aware until recently that either Biett or Devergie had described them. In three of my drawings the streak is shown running down the course of the trunk of the ulnar nerve, exactly as described by Biett. In three others the streak runs from the anus to the foot as in Devergie's case. In most instances those streaks not congenital begin in early life, but in a few, as in Devergie's case, they are first observed in adults, and appear to be part of the eruption of lichen planus. The congenital or childhood cases appear to be sometimes at least parts of the eruption known as Ichthyosis herpetiformis. To these isolated streaks the term herpetiformis is not wholly applicable, for the changes are located not by the distributed filaments of a nerve, but are placed over its trunk. This is remarkably the case in those in which they are said to be located by the course of the ulnar nerve. In those which run down the back of the thigh no nerve can be identified as corresponding in any way with the location of the changes.

One of my portraits showing these bands is a photograph from the St. Louis' Hospital. I would venture to propose that these streaks, as to the true nature of which there is as yet much doubt, be known conventionally as

BIETT'S BANDS.

To Biett undoubtedly belongs the credit of having first mentioned them.

* See Museum Catalogue, Part I., pp. 24 and 94.

No. CCXII.—*A Tuberous Urticarial Eruption tending to end fatally and accompanied by Vesications.*

A lady, named H——, aged 52, consulted me on account of a very peculiar form of urticarial eruption. She was pale, thin, and very sallow, and had suffered much from indigestion. About two years before she had had some kind of eruption on her shoulders which she had attributed to a chill. Her present eruption had commenced, however, only about ten months ago. She was then living in Yorkshire. She had much treatment without relief, and five months later came to reside in London, but still obtained no benefit. The eruption had, she said, much interfered with her health and made her feel frightfully depressed. The eruption consisted of a sort of tuberous urticaria, each wheal being attended by solid infiltration of the skin and subcutaneous tissue under it. On the middle of many of the wheals vesication had occurred, and in some even ulceration had resulted. Groups of new spots made their appearance every few days and the old ones disappeared. She spontaneously stated that the new spots "would itch and sting exactly as if I had been bitten." It was a remarkable and very definite fact that the eruption was not arranged symmetrically. Many of the newly-developed wheals had the appearance of a puncture in the centre. These appearances, taken with the fact of non-symmetry and the constant occurrence of new spots, made me suspect, in spite of its apparent improbability, that the eruption must, after all, be due to bites. The patient had, however, been exceedingly careful as to cleanliness and change of clothing, and the eruption had persisted, although she had changed her place of residence. I advised her to come into a Home in order that she might be watched and her clothing carefully examined. The irritation had been so great and its effect on the health such, that it appeared not unlikely that the case might end fatally. The patient's limbs and body were covered with inflamed wheals, vesications and ulcers in various stages of progress.

I saw the subject of the above narrative only once, but I

heard some months later that she had died, worn out with irritation.]

No. CCXIII.—*Almost fatal Epistaxis in a Pregnant Woman.*

I was sent for in great haste one morning in May, 1871, to see a woman who was said to be dying from epistaxis. Dr. J. Morrison, whom I met in the case, told me that he had been attending the woman during the last twenty-four hours, and that, in spite of all remedies, the bleeding had continued to such an amount that he feared a fatal result.

Our patient was a married woman aged 36, and in the sixth month of pregnancy. The hæmorrhage had commenced suddenly about two o'clock on the morning of the day previous to my visit. After a day of rather unusual exertion, she had gone to bed feeling quite well. At two in the morning she awoke, and, having occasion to get out of bed, the bleeding commenced as soon as she assumed the erect posture. There was a history that ten years ago, in the sixth month of a former pregnancy, she had had a severe attack of epistaxis, but that it did not last more than half an hour. On that occasion it commenced whilst she was walking in the street. In girlhood she had been liable to epistaxis, but not to any remarkable extent. At the time of my visit our patient was in collapse from loss of blood, and could barely speak. The anterior nostrils had already been plugged, and no bleeding was then going on. I directed that she should be propped up in bed with the head as high as possible, care being taken that she did not faint, the feet put in hot water, and ice applied to the back of her neck. The bleeding did not return. Three months later the woman was safely delivered of twins, and without any unusual amount of hæmorrhage. Three months after that she called on me, in tolerable health, though her pale complexion still bore evidence to the loss of blood which she had sustained. Epistaxis does sometimes end fatally, especially, I think, in women.

In connection with the case of Mrs. M——, who herself

suffered from it, I was told that two sisters had died of uncontrollable nose bleeding. One was forty years of age.

I have the utmost faith in the line of practice pursued in the above case, that is, heat to the feet and cold to the head, and if possible the sitting posture. In the above we did not dare to make the patient sit up, but simply drew the feet over the edge of the bed.

No. CCXIV.—*Leprosy Stones in Fiji.*

The advocates of contagion as the sole cause of the spread of leprosy, eagerly adopt all assertions as to its introduction into different countries during comparatively recent times. They tell us of the Crusades, of invading armies, and of immigrants. Those who, on the contrary, believe that the disease is due in the main to peculiarities in food, and that it is capable of originating spontaneously where favouring conditions exist, hold that it has been present from prehistoric times in a great number of distant parts of the world. Evidence of this latter point has been difficult of attainment in uncivilised communities, which have preserved no written history.

In reference to these latter, proof of a somewhat unexpected kind has been recently supplied. In an important paper read by Dr. Corney before the Folklore Society on Leprosy Stones in the Fiji Islands, accounts are given of the existence of stones which imply the presence of the disease in remote ages and in many distant places. These stones appear to have been selected on account of their peculiarly spotted surfaces. They are objects of a superstitious reverence in several different directions. They were in some instances supposed to be the abodes of the spirits of dead lepers, and to have the power of communicating leprosy to the living. It was also necessary to propitiate them in order to be secure against the malady.

Dr. Corney's essay proves that leprosy is at the present time prevalent in the Fiji and neighbouring islands, and also that it has been so from very remote times.

No. CCXV.—*Infection of Anthrax from Foreign Oats.*

A case of much interest in reference to the importation and spread of anthrax was the subject of an action for damages a few weeks ago. A number of horses in two different and distant stables, under the same ownership, had died of anthrax. The facts, when investigated, led to the conclusion that they had been infected by some Turkish oats which had been supplied to both. Experiments with the oats (which were admitted to be in a very dirty condition) supported this inference, for animals inoculated with an infusion died of anthrax. A large majority of the horses fed with the oats did not suffer, and it was given in evidence that other stables had been supplied with the same and no complaints had been heard of. It was suggested that only those horses which had sores on their lips, mouth, or other parts of the mucous membrane had contracted the disease, the bacillus being incapable of entering sound tissues. It was further asserted that the spores might remain quiescent but retaining life for several years.*

The case afforded a remarkable illustration of the application of modern science to the elucidation of an occurrence otherwise inexplicable. The evidence given was good and conclusive.

No. CCXVI.—*Double Skulls.*

The Vrolik Museum (Amsterdam) is very rich in malformations. There are several examples of double skulls; two are those of sheep, and, from their size and ossification, apparently those of animals which lived to adult age.

This museum, commenced, I believe, by Hovius in the middle of the last century, and enriched by the industry of Vrolik in the early part of the present one, is of great value. It is, I am sorry to say, now somewhat neglected and not so accessible as its importance demands. I had hoped to find in it the unique skeleton of the polydactylous dwarf described

* See *The Times* for Saturday, October 7, 1895.

by Theodore Kerckring, the illustration of which was reproduced in ARCHIVES, Vol. IV., Plate XCII. I was, however, unsuccessful, nor could I obtain any information respecting it, although my friend, Prof. Tilanus, did his best for me.

No. CCXVII.—*Posterior Dichotomy in a Pig—Stuffed Specimen.*

The Ipswich Museum contains an interesting specimen, stuffed, of a dichotomous pig. It is a young animal and probably died soon after birth. It is very neatly stuffed in the attitude of eating from a trough. It stands well on two forelegs and four hind ones. Its head and fore-quarters are of natural appearance, but a cleft extends through the middle of its back dividing its hind-quarters into two, each having well-formed limbs and tail, but smaller than the fore part of the animal. These four hind limbs are well placed under the animal, and it looks as if it could have walked easily. This being so it is of much interest to note that duplicate fore-limbs rise from the animal's back over its shoulders and project upwards.

No. CCXVIII.—*Skeleton of a Dichotomous Kitten (posterior).*

In the Vrolik Museum in Amsterdam there is an example (a skeleton) of posterior dichotomy in a kitten very similar to that in the pig at Ipswich. The kitten has but one head (which is well formed), whilst the posterior part of its body is duplicate. It has eight limbs, and, as in the case of the Ipswich pig, two of its forelegs are on its back. Its hinder legs, however, are not placed under its body, but its two pelves look towards each other. It could not have walked.

NOTES ON YAWS.

LORD ACTON, in his recent address from the chair of Modern History at Cambridge, urged that it was in some sense the duty of intelligent persons to investigate the evidence and form opinions in reference to the characters and actions of the distinguished men in the past. Our political and social reformers are never tired of urging the same claim in reference to the problems of the day. The man who does not record his vote, on one side or the other, is now looked down upon by both, and is liable to be dubbed by some uncomplimentary nickname. Whilst by no means willing to admit that, respecting many historic and political questions, neutrality is not the most reasonable attitude of the partially informed mind, I should yet like to evoke a little of this zeal for some matters in which our own profession is concerned. There is a certain amount of claim, on the part of the public, that a professional man should not restrict himself too closely to his every-day life, but should endeavour to arrive at just opinions on all the moot questions which concern his department of knowledge. He should know—most do know—the great facts as regards the history of inoculation and vaccination; those in respect to rabies; the history of epidemics; the nature of malaria and the like. On these and many similar subjects he should be able to direct popular action. The duty, for instance, of having an opinion as to the cause of Leprosy seems to me far more cogent, in the case of the surgeon, than that of forming a correct estimate of the character of Mary Queen of Scots, or even than being able to judge as to the comparative merits of Des Cartes and Newton; to say nothing as to choice between Lord Rosebery and the Duke of Devonshire. Not for a moment do I underrate the paramount importance

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of national and political questions, but I realise that, to the Englishman, at any rate, there may occasionally arise a not unreasonable impression that many details are better left in the hands of those who have made it their business to study them. At any rate, this I will venture to urge strongly, that if a medical man has leisure he will find much instructive, interesting and profitable work to be done in the outlying problems which concern his own profession.

Feelings such as I have just endeavoured to express have impelled me not unfrequently to deal with topics in the pages of my ARCHIVES which may possibly not have been considered to concern directly the members of the British profession. For this I make no apology. Rare diseases; very exceptional manifestations; the opinions of physicians long ago dead; the accounts of maladies which exist only in distant parts of the world, are each and all often of the utmost value to us, as conducing to a sound and complete knowledge of our art. It is only by the inclusive study of all these that we are enabled fully to discharge the duties which the profession of medicine lays upon all those who embrace it.

Perhaps the topic which at the present day most presses for solution is the Cause of Leprosy. A correct decision respecting it concerns probably the happiness of more human beings, and that to a more profound degree, than any other question now occupying the medical mind.* The nature of Yaws and its proper treatment is one which is only second in importance. It is second, for the disease prevails far less extensively, and a knowledge of its cause would probably be far less influential in respect to its prevention and cure. Still, it is the source of an enormous amount of misery, much of which might doubtless be prevented. Although seldom or never recognised in England, it prevails extensively in many of our Colonies, and a humane Colonial Executive has recently spent much money in the endeavour to elicit information respecting it. Yet a large proportion of the British profession would be willing to avow utter ignorance on the subject, and by some writers on medicine, although

* See a "Conversation" at p. 92.

West, Newman chromo.

PLATES CXLVI. & CXLVII.

THE ERUPTION OF YAWS (AN EARLY FORM).

THESE two Plates, which have been executed from photographs, are intended to illustrate an early form of eruption in Yaws. They show the front and back aspect of the same patient, apparently a lad or young adult. The eruption consists of small patches, for the most part isolated, which occur freely over the trunk, limbs, face, and even the scalp. They are arranged with fair symmetry, and are tolerably uniform in appearance, with the exception that there is one in the left groin much larger than the rest.

It would be unsafe to use these portraits as demonstrating anything more than the location, size of patches, and generalised character of the eruption in the secondary stage of Yaws. The portraits do not show enough detail as to the condition of the crusts to make any description trustworthy. It may be stated, however, that in this stage the eruption of Yaws is described by authors as presenting different characters in different cases, being sometimes papular, sometimes scaly, and in others attended by greater or less tendency to the growth of raspberry-like granulations. As a rule, however, the latter are not seen till a later stage of the malady. The eruption here shown is stated to occur usually at about two or three months after the date of contagion.

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West, Newman chromo.

PLATES CXLV. & CXLVIII.

THE ERUPTION OF YAWS (A LATER FORM).

THESE two Plates, executed, like the two preceding, from photographs, show a later condition of the eruption of Yaws. Large masses of elevated and rupia-like crust conceal fungating granulations. In many places the sores have coalesced, in consequence, no doubt, of spreading at their edges.

This stage of Yaws may be reached a few months after contagion, and may be indefinitely prolonged. The generalised eruption, however, comes to an end eventually, and as a rule the patient regains health. Disease of bones, ulceration of palate, &c., may follow.

It may be assumed that in these portraits an unusually severe case is depicted.

West, Newman chromo.

such is not openly avowed, it is very adequately revealed. Yet it is a disease which may be easily studied, and about which there exists an abundant literature.*

I purpose to devote a few pages in this and my next ARCHIVES to the attempt to give a clear view of this important subject. As a beginning, the four Portraits appended are offered as affording a fair pictorial representation of the most conspicuous appearances presented. These portraits have been made from photographs taken in the West Indies. They have been inspected at the Clinical Museum by many familiar with the disease in its haunts, and have been accepted by all as affording good representations of it. I regret that I am not able to give the life history of any one of the patients, but this is not of so much consequence, since the portraits are offered as being typical rather than as elucidating single cases. The original photographs were, I believe, given me by Dr. Nichols some years ago.

On the present occasion I shall not trouble my readers with more than a few extracts from authors, with brief comments. I have already, under the head of Framboesia Cromwelliana (page 23), made incidental allusions to the subject.

Dr. Wright claims that Yaws is an Exanthem.

Dr. Wright in his Dissertation on Yaws, to which I have elsewhere referred, has the following remarkable passage: "Dr. Cullen places Framboesia in Class III., Cachexiæ, and Order III. Impetigines, next to Lepra, whereas it ought to have been arranged under Exanthemata, next to Variola,

* Whoever wishes to master the subject of Yaws should begin with the chapter in Hirsch's Handbook (New Sydenham Society). He may then read the Blue Book containing Numa Rat's researches, and lastly the well-illustrated Report (also a Blue Book) just published by Dr. Nichols. The New Sydenham Society is about to reprint (as one in a volume of short monographs) the brief but most valuable prize Essay written by Dr. Maxwell in 1838. This Essay, which has the advantage of having been written before the days of the present controversy, is a mine of facts, and will enable any one who reads it to form for himself a just opinion as to the nature of the disease under consideration. Until the Society's reprint shall appear it is, however, inaccessible, as there exists, so far as I know, only the single copy which is in the library of the College of Surgeons.

because, like the small-pox, it has its accession, height, and decline; like the small-pox, it is taken by inoculation; and, when a person once passes safely through the yaws, he cannot again be infected by any means whatsoever. This fact is so well established that a negro is valued one-third part more by his having formerly had the yaws."

Had the author of this statement only recognised the identity of yaws with syphilis he would have anticipated, by half a century, the observation that the latter ought to rank with the exanthems.

He denies that it is Syphilis. But why?

As a matter of fact, Dr. Wright denied that yaws was syphilis. But why? Because he thought that syphilis never got well unless treated with mercury, and that it might, "as we all know," be contracted repeatedly by the same person. In addition to these reasons, he adds, "It is true that the yaws affect the bones, the nose, and the palate, like syphilis, and admit of similar cure; but in syphilis there are neither eruptions nor fungi, as in yaws, except on the privities, and then only in form of warts." Here again it will be seen that there is a large misconception of the facts as to syphilis.

On the hypothesis that yaws is syphilis, the above statement by Dr. Wright bears strong testimony to the possibility of the spontaneous disappearance of both. It is not, however, stronger than non-mercurialists would still be prepared to offer in the case of syphilis. Every one must indeed admit that in many cases syphilis does pass away without any specific treatment. Yaws, however, did not, in Dr. Wright's experience, always get well; for, in another place, he writes respecting medical men having charge of negroes suffering from yaws: "The risk such gentlemen run is very great, for should a medical man contract this filthy disease his fortune and future prospects are ruined. He must be secluded many months from society, and if he at last escapes with his life it is well."

Although most of our Insurance Offices have, I believe, agreed to disregard a history of past syphilis, provided the

insurer has regained good health, I have not heard of any which consider such history an advantage. Yet were sexual morality in England what it was amongst negro slaves in the West Indies, and were the risks of accidental contagion as great, it would certainly be an advantage to have got well through syphilis. Clearly Dr. Wright expected that all would suffer some time or other. In describing the planter's arrangements as to his Lock Hospital for yaws, one of which, he states, was to be found on every well-regulated estate in Jamaica, he says that it is to be placed under "a careful and discreet matron who has herself formerly gone safely through the disorder."

Dr. Wright's treatment of Yaws.

As regards the treatment of yaws, Dr. Wright recommends cleanliness and attention to the general health; but adds, "towards the decline, if the disease does not go off kindly, mild mercurials may be given with safety and advantage. They should be administered in small quantities and not to occasion ptyalism." This is exactly as many would now write of syphilis.

Dr. Adams records as Yaws a case which was clearly Syphilis.

In his well-known work on Morbid Poisons, Dr. Joseph Adams has a chapter on yaws, and gives us, in it, a detailed record of a case which he observed in a *white subject*. Dr. Adams was at the time resident in Madeira, and he had not himself seen yaws in its native place, but he availed himself of the confirmatory diagnosis of Dr. Wright,* who had done

* The "Dr. Wright" here referred to was probably Dr. William Wright, F.R.S., from whom I have quoted above. Dr. Wright had resided many years in the West Indies and wrote some valuable essays on botanical and medical subjects. Amongst his papers (collected and published with a biography in 1828) was a "Dissertation on Yaws." Dr. Adams's statement is: "Enough of the pustules still remained in a state of partial suppuration to satisfy Dr. Wright, who at this time touched at our island in his return to the West Indies, that the disease was truly yaws." The case occurred in the last months of 1793, whereas Wright's biographer asserts that he returned finally to England in June of that year. There may

so. As my contention is that yaws is neither more nor less than syphilis, and that the case adduced by Adams was one of syphilis only, I shall in the following abstract of it quote the exact expressions used, in order that I may not unintentionally give a colour of my own to the narrative. I ask, however, that the following notes may be read with that suggestion in mind.

A young nobleman who had been in the West Indies, in the Danish naval service, was put ashore at Madeira, being too ill to continue his voyage. It was in the latter end of September, 1798. He was feverish and had pain in his throat. Under a diagnosis of intermittent, bark and other tonics were given, but with only very partial success. He next took a grain of calomel daily for ten days, during which his mouth became slightly affected and his fever completely left him. It returned, however, as his gums recovered, "and then followed an universal pimply eruption over the face, and inflammation of the throat." The eruption became quickly universal excepting the hands and feet. "The throat became ulcered, and the glans penis was covered with several small, but not painful, ulcers." The eruption in the course of a week or two developed into one closely resembling small-pox, excepting that the pustules were not umbilicated. "The throat had precisely the appearance of a venereal sore throat, but was more painful." "The continual increase of the pustules, after those of small-pox should have scabbed, precluded all doubt concerning that or any similar morbid poison."

"By the application of aqua phagedænica,* the ulcers on the glans penis soon became stationary, put on a better ap-

however, be some error in the dates. Dr. Wright's dissertation on yaws had been published in a Latin translation as an Inaugural Thesis by a ward of his, for whom indeed he had written it. It is quoted in Latin by Dr. Maxwell and attributed to Dr. Ludford, but Dr. Wright's executor having found the original in his desk and in his own handwriting, naïvely prints it in English amongst Dr. Wright's collected essays. Dr. Ludford had clearly nothing to do with it beyond procuring its translation into Latin and presenting it, as his own production, to the University of Edinburgh.

* *Aqua phagedænica* is an old name for "yellow wash," a solution of perchloride of mercury in lime water. Not improbably it was used also for the ulcers on the skin, and had an important share in the cure.

pearance, and healed. It was therefore evident that they were not venereal."

"The eruptions continued to increase in number and size to such a degree that the soreness, abstracted from the pain which was confined to those on the face, rendered life scarcely tolerable." "Before the end of the month my patient counted, besides a number of smaller ones, fifty-six large sores, some of which, being of an oval form, were not less than two or three inches in their largest diameter." "The state of his throat prevented his swallowing even liquids without very great pain."

The subsequent narrative records that the patient had a most severe febrile illness, so that Dr. Adams had to see him two and even three times a day. His throat was so much ulcerated that it was feared that the uvula would be lost. When, however, healing took place the parts had kept their natural form. No special treatment was adopted, excepting the use of bark in enemata, as he could not swallow it. After several exacerbations slow recovery took place, and at the end of six months most of the sores were healed and the throat was "nearly well." No further particulars are given.

The paper in which this illustration of "Yaws in a European" is recorded was read on June 30, 1800, before the London Medical Society. It would be tedious to enter into detail on the symptoms which Dr. Adams believed to justify his diagnosis. They were the severe pain in the throat, the subsidence of the eruption without mercury, and other similar ones. The citation of them as distinctive shows much want of familiarity with what we now regard as common occurrences in the course of syphilis.

Is it not clear that this case was one of syphilis in which, under the erroneous diagnosis, all treatment was omitted and consequently unusual severity encountered? The eruption was the variola-like type now well known. All the rest of the phenomena were also those of syphilis. The chancres healed and disappeared under yellow wash. The eruption never assumed the raspberry character at all, and it was

ten months since the patient had visited a country where he could have contracted yaws.

The case has collateral value, as showing how severe the secondary stage of syphilis may be when specifics are not used, and next as proving that, in spite of their disuse, a very severe form may get well in time.* The possibility of spontaneous recovery from syphilis must, however, be freely admitted, and to this day there are able practitioners in Edinburgh and Christiania who still hold that it is best to avoid mercury. The remarkable difference in the opinions held in the beginning of the century from those which we now entertain are disclosed in the following sentence:—"The venereal is a poison of which the constitution is for ever susceptible, and which it has no power of curing in itself; consequently the disease is kept up for ever, or till a more powerful stimulus is applied: after which, on a fresh application of the contagion, the constitution is found as susceptible as before."

Dr. Adams was a careful observer and a most sagacious writer. It is very necessary, in reading the early literature of Yaws, and more especially the reasons which caused its separation from syphilis, to remember that these were the doctrines of the day.

* I have adverted in the footnote on page 80 to the possibility that mercury was used as a local application, and more efficiently than was suspected.

RECORDS OF DEMONSTRATIONS AT THE CLINICAL MUSEUM.

THE Demonstration-conferences take place every Tuesday at four o'clock, and are open to all members of the profession without any ceremony of introduction. We have had during the past quarter many cases of unusual interest. I propose here to give a brief *résumé* of some of the more important of them. In so doing it will be convenient to make no attempt to keep to dates, but to take together cases which are of similar character.

On November 12th we were fortunate enough to have together two examples of the rare malady known as Kaposi's disease, or Xerodermia Pigmentosum. One of them was a typical example of the affection, but the other presented peculiar features. The first was a girl aged 8, sent by Mr. Willett from St. Bartholomew's.* She was the only one in a family of three who suffered, and, as usual, there was no history of inheritance. The stage of freckles had been noticed at the age of two, and the conditions had been progressive ever since. The face and neck, with the hands and forearms, were the parts chiefly affected. The legs were involved in a less degree, the skin as high as the knees being dry, and showing some pigment spots. The case, although well marked, did not show any of the more severe conditions of ulceration, &c.

The second case was a lad aged 21, brought by Mr. James Startin. His face was covered with pigment spots and stigmata. The skin was tight, and ulcerated in places. The state was, however, complicated by a general slight xerodermia, with a liability to the production of blisters when the skin was rubbed. In the palms the skin was dry and

* Mr. Willett has since been good enough to present to the Museum an excellent photographic portrait.

thick, the fingers were not freckled, but more or less excoriated, and all the nails had been lost. The legs were red and excoriated, and there were large bullæ on the knees and shins. The condition was stated to have been first observed, as a liability to blister, at the age of two. One of his brothers was stated to be severely freckled, and a sister moderately so. In this instance the tendency to pigmentation was much less marked than in typical cases, and liability to blister was much greater. The parts affected were, however, those which are exposed to local irritation from sun, wind, &c. Unlike what occurs in pemphigus, there were no bullæ on the trunk or the more protected parts. That the case properly belonged to the Kaposi group I could feel no doubt; the real malady being a congenital defect in the organisation of the skin, rendering it liable to suffer from exposure.

In connection with these cases, I showed a series of portraits illustrating Freckles, Kaposi's disease, and the vesications which occur in connection with Summer Eruption. Of these the Museum possesses a most instructive series in which the various forms and stages of these allied conditions can be well traced.

Prurigo Hiemalis of Lichen Type.

The subject of this case is a man aged 26, of whom I possess two portraits, and who has been occasionally under observation since 1888. He is usually quite well through the summer. When he came before us on the present occasion his trunk was covered with lichen papules, for the most part discrete, but in some parts arranged in rings and groups. Much difficulty had occurred in making a diagnosis of this eruption in the first instance. It had persisted for many months without benefit. It is now, however, quite clear that it is an eruption which occurred in connection with cold weather. Although it occupies exactly the vest regions, yet the patient does not associate it with any change of clothing. The hands and feet, and for the most part the arms and legs, were exempt.

Multiple Sarcomata in the Subcutaneous Lymphatics.

The subject of this case was a woman of fifty-two, sent to us by Dr. Dowding, of Wanstead. She had a growth, possibly the primary one, three inches long on the left border of the sternum. This was of a year's duration, and firmly adherent to the bone. Between this growth and the armpit were a large number of small, painful nodules, freely movable, and probably having their seat in the trunks of lymphatic vessels. No lymphatic glands were enlarged.

Primary Melanotic Sarcoma of Skin.

In this instance I showed only the specimen, after excision. It consisted of a coal-black nodule not larger than a pea, which I had excised from the temple of a young lady of nineteen. It had been growing for three years. The family history was of importance. One of her father's brothers had died, at the age of nine, of cancerous growth in one arm and internal organs. He had been under the care of Sir Benjamin Brodie, who had proposed to amputate, but had been deterred by discovering internal disease. The patient's paternal grandmother and a maternal aunt had also died of malignant growths.

I took occasion in connection with this case to express a firm belief that the tendency to cancerous action might be transmitted hereditarily, and that we must not expect it to show itself by the production in different individuals of precisely the same form of cancer. Under the law of transmutation in transmission, a proclivity derived from ancestry may result in cancerous action in some different organ or tissue. The precise form taken by the new growth will depend upon the precise tissue in which it begins. I also said that whenever cancerous action shows itself in young persons we may be confident that there is inheritance. In this instance two young persons, one in each generation, had suffered.

Syphilitic Psoriasis of the Nails.

I hold that the form of nail disease which goes with

common psoriasis affecting the skin is usually well characterised. It consists in a loosening of the nail from its bed, at its sides and free border, and in the accumulation of dry epidermis under it. This accumulation makes the loosened part of the nail look opaque and dirty, but the nail itself is not roughened or broken. A bristle or fine probe may easily be pushed under the loose nail. The lower part and the lunula are not affected, and the surface remains smooth. I was able to demonstrate these conditions in a most characteristic form in the case of a gentleman who attended in order to show his nails on November 12th. All the nails had suffered, and all were exactly alike. The distal third on every finger was loose from its bed. The condition was of syphilitic origin, for the patient had had a chancre about two years before. He was in all other respects well. I remarked that although this was a typical example of psoriasis of the nails, it would be a great mistake to suppose that all forms of syphilitic affections of the nails are the same. On the contrary, the varieties of syphilitic onychitis are as many and as marked as those of skin eruptions.

On Difficulty in the Recognition of Syphilitic Eruptions.

I have frequently taken opportunities of demonstrating that it is often not possible to diagnose syphilitic eruptions by their appearances alone, and that there are many cases in which, unless the history helps us, we must leave the question in some doubt. Self-confident persons may venture confident opinions, and occasionally they will have the pleasure of being emphatically right; in others, however, they may be wholly wrong, and much injustice and social misery may be caused by their errors. It is not possible, excepting by the history, to diagnose the syphilitic forms of Bazin's legs from those which are strumous. The proof of this is illustrated by the fact that until quite lately all these cases were called syphilis. We have had before us many cases in which nothing but the history of the case enabled us to feel certain that the eruption was not syphilitic,

and a few, much fewer, in which some hesitation was felt in pronouncing definitely respecting an eruption which was really specific.

One of the latter occurred in the person of a young girl of sixteen, sent to us by Mr. Waren Tay, who was covered with characteristic patches of common psoriasis. The patches were very large, scale-crusted, abruptly margined, and quite dry. This occurred on the backs of the arms as well as their fronts. After a full and careful inspection, I believe most of us remained in doubt. My own diagnosis in favour of syphilis was based upon the rapidity with which the eruption was said to have developed, its abundance, and the severity with which it affected the face. The girl was covered from head to foot, and it was said to have all come out within two months. There was no sore throat, and we could not, on account of her youth and other circumstances, make any direct inquiry as to primary symptoms.

The diagnosis of syphilis was a fortnight later confirmed by the appearance of condylomata at the anus. What is usually called syphilitic psoriasis often bears little or no resemblance to psoriasis vulgaris, but in this instance the simulation was complete.

In another instance, a young man of twenty-seven showed us a copious eruption of copper-tinted papules, which covered his abdomen, chest and back. There were but few spots on the limbs. At first sight no one would have doubted that it was syphilitic, but on closer inspection it was seen that many of the papules had become smooth and polished. There was an absolute absence of history of syphilis, and neither the genitals nor the throat showed any suspicious conditions. The eruption had itched, and in some places had been scratched. It was of only a few weeks' duration. In spite of its absence from the parts more usually affected in lichen planus, Mr. Hichens, whose patient the man was, and myself agreed in recognising that malady.

The Crateriform Ulcer—Microscopic Examination.

Several interesting examples of the crateriform ulcer, or acute crateriform epithelioma, have come before us. One of the most characteristic of these was in an old woman,* who was sent to us by Dr. Stocker, of Forest Gate. She was seventy-three years of age—a thin, withered woman. The growth had begun about fifteen months ago. Her father had died in old age of cancer of the lip. The ulcer was in one of its most usual sites, *i.e.*, just above the eyebrow. It presented most characteristic conditions—thicky, bossy, smooth edges slightly nodulated, and a black adherent crust in its middle. This crust and the hollow which it concealed had formed without any obvious ulceration, for the patient stated that there had never been any discharge from the sore. The base of the growth was well circumscribed, but not as large as a halfpenny. It was placed on skin which was not in the least inflamed. It had not been painful, and was quite unattached to the bone. A photograph of the patient was taken. Dr. Stocker subsequently excised the growth, and was good enough to send it to us for examination. Slides prepared by the Clinical Association were exhibited on June 25th, the report being as follows:—

"This specimen consists of tissue so arranged as to give the aspect of a squamous-celled carcinoma. The tumour is remarkable for the want of surface-hypertrophy of epithelium. The epithelium passes from the flat surface, where it is still comparatively normal, over a rounded elevation where it becomes gradually thinned out. The elevation is caused by proliferation of the connective tissue elements and round cell exudation. Through this mass of tissue are scattered masses of epithelial cells arranged in a manner characteristic of the deeper prolongations of a squamous carcinoma. On advancing farther into this tumour-like mass, its surface epithelium becomes more thinned out, while the deeper prolongations of epithelium become more pronounced. Epithelial 'cell

* See Clinical Journal.

nests,' characteristic of overgrowth of squamous epithelium, make their appearance.

"On passing over the edge of the rounded tumour the surface epithelium disappears, the surface being covered simply by a thin fibrous membrane—resembling a thickened basement membrane. The surface shows small collections of exudation in position. Gradually this surface membrane disappears, and the exposed connective tissue surface is evident. Projecting into the deeper structure from all parts of this denuded surface are the prolongations of epithelium already described."

This case afforded the opportunity for a comparison of the patient's condition with that shown in a series of portraits contained in the museum, the close similarity of one case to another being most remarkable. I drew attention to a very important feature in these acute epithelial ulcers—that they appear to have but little infective power, and do not return after a free excision, nor do they, so far as I have observed, infect the lymphatic glands early. If neglected, however, I believe that glandular disease does follow.

ILLUSTRATIONS OF YAWS AND ALLIED MALADIES.

DR. NUMA RAT (formerly of Antigua, now of St. Christopher) has sent me several photographs illustrating Yaws. I print verbatim Dr. Rat's graphic descriptions of them, and must leave it to my readers to judge whether they do not describe the phenomena of syphilis. The photographs can be seen in the Museum.

"The photo of the young negro in the same picture shows a squamo-papular eruption on the extensor surfaces of his forearms. This is what is popularly called in the French West Indies 'pian gratelle.' It is considered to be the yaws eruption which has not proceeded to fungation, and, instead of developing into tubercles, has remained at the previous stage of papules. Papular eruptions, however, on the extensor surfaces of both the legs and arms, especially about the elbows and knees, are so common among negroes that one hesitates to assign such eruptions to yaws or any other particular disease.

"The photograph of the girl shows a condition which I have often observed in places in which yaws prevails. Over the arms are scattered shiny scars left by ulceration which has destroyed the true skin and produced depressions covered by a thin skin like that of a tomato in consistence, this thin skin being either smooth or wrinkled, lighter in shade than the adjacent parts, and dotted here and there with white spots, the result of depigmentation. Between these are other scars of keloidal appearance, the most marked of which is over the radial aspect of the right arm towards the origin of the pronator. The light-coloured lumps are dome-shaped or conical tubercles, the skin over which has lost its pigment. The majority of these are ulcerated at their summits, the ulcers being covered with buff-coloured or brownish crusts. The conical tubercles with the ulcers at their apices resemble, when they have lost their crusts, diminutive volcanoes, the ulcers being the craters. Such a volcanic tubercle is shown over the inner humeral condyle on the left arm. A dome-shaped tubercle may be seen over the left deltoid, and next to it a keloidal scar. The right index and middle fingers are in a condition of dactylitis; so is the left index, the middle and ring-fingers on that side having proceeded beyond this stage, and being marked by fungating conical ulcers. On the back of the left hand is seen a corrugated shiny scar. The scars just below the insertion of the left deltoid were left by two vaccine pustules. These scars are corrugated and shiny

like those on the forearm. Now, as the girl was vaccinated before she exhibited any skin disease, the probabilities are that there was something in her constitution which predisposed her to such peculiar ulcerations with their peculiar resulting scars.

"The girl's legs and feet are more or less in the same condition as her arms and hands. With the exception of the left shoulder, the eruption is limited to the limbs, below the knees and elbows. No scars are seen about the head and neck or the trunk. A few scars like those on the arms are to be observed on the buttocks and backs of the thighs. It is to be noticed that, although some tubercles and scars exist on the flexor surfaces of the limbs, the eruption has preferred the extensor surfaces for its development.

"The history of the girl is that she contracted yaws last year, and that she was subjected with several others suffering with the same disease to the treatment practised by the native bush-doctors, this treatment consisting of decoctions of various herbs and the external applications of the caustic juices of certain plants. The other patients were cured, but in her the yaw eruption proceeded to ulceration and developed until her limbs became covered by the scars and tubercles shown in the picture. Two medical men who have seen a great deal of yaws, and to whom I showed the case, considered it to be one of yaws 'improperly treated,' whatever that may mean. Another who saw it thought that it was syphilis. (The nose-bridge is rather depressed even for an African, but there are no other signs of syphilis beyond the skin affection.) I have no doubt that if I had continued to show the case to other medical men, some would have declared it to be scrofula.

"I prescribed iodide of potassium with bichloride of mercury. She took that mixture between the 25th of October and the 25th of November, and when she returned to me on the 25th of November the tubercles had all disappeared from both her legs and arms. All the volcanoes and their craters had vanished, their sites being marked by small, shrivelled, dark-brown scabs resting on healthy skin. The dactylitis had departed. The only part in which some ulceration remained was the left ring-finger, which had, however, wonderfully improved. Such was the astonishing improvement in the patient, that the mother in an outburst of gratitude brought me six cocoa-nuts as a present.

"The other photo shows two deep scars in a woman's thigh left by two ulcers. You will also notice a similar scar a little above her left patella. She consulted me about a huge cavity produced by ulceration in the back of her leg in the lower half of the popliteal space. The scars on other parts of her legs had resulted from similar lesions. I prescribed for her an iodide and bichloride mixture. Under this treatment, after taking twenty-four ounces of the mixture, she declares herself getting on splendidly. The cavity is filling up rapidly with healthy tissue."

CATECHISM OF SURGERY.

No. CCVII.—*On the Causes of Leprosy.*

A CONVERSATION.

Ille. You have in former years expressed a strong opinion that the efficient cause of Leprosy is the eating of fish. May I ask if you still hold to that creed?

Ego. I certainly do, and with increasing firmness of conviction. The attitude of my mind is such that I cannot conceive of any one, who is master of the facts, entertaining material doubt about it. There are many matters of detail still to be settled, but as to the main conclusion I have no misgiving.

I. What do you mean by "master of the facts"? I believe that you yourself have never lived in a leprosy district.

E. It is quite true that I have never resided in a leprosy country, and I find that those who have done so sometimes seem to think that such experience alone can justify a man in having his own opinions. An exclusively local experience is, however, often somewhat misleading. You cannot see the wood for the trees. I have visited all the large leper hospitals in Norway. I was taught the diagnosis of leprosy by Drs. Danielson and Boeck themselves. I am fairly well read in leprosy literature, and during the last twenty-five years I have seen a fair share of the leprosy patients who come to England. I do not wish to boast, but really I feel as if I were as likely to form just conclusions as any one who, having had the advantage of long residence in a leprosy district, may yet probably know the disease only in connection with one locality. I have listened to all that many observers

having local experience could tell me, and as a result am sanguine enough to believe that I am able to take a wide and fairly accurate view of the whole subject.

I. You have not answered my question as to the sense which you attach to the phrase, "Master of the facts."

E. I mean one who knows all the facts, and not only knows them, but enjoys the use of a memory sufficiently retentive to keep them all in mind at once, and thus allow them their proper force in controlling and balancing each other. Some of my well-informed friends with whom I have discussed this matter appear to me to lose sight of one class of facts while they are discussing another. Those who can keep the whole mass of apparently contradictory facts in mind at once will, I feel confident, come to the same conclusion that I have done that fish-eating is the efficient cause of leprosy.

I. Have you in any way modified your belief in this creed of late years?

E. Formerly I used to suspect all fish, but of late I have come to the conclusion that it is only raw fish, and more especially dried, or partially salted fish, eaten raw, which is to blame. I believe that if you could enforce the adequate cooking of all fish that is eaten, whether dried, salted, or fresh, that leprosy would cease. The remedy, you see, is simple enough, and a trial of it would not involve any loss of food to the community.

I. You have, of course, read the Report of the Leprosy Commission in India, and the Prize Essays which have recently appeared under the auspices of the Prince of Wales's Committee on Leprosy?

E. I have read them carefully. In fact I have been a member of the Committees under which they have been produced.

I. You do not find in any of them any degree of acceptance of the fish theory?

E. Not in its naked form. I fear that I cannot as yet count a single disciple. There is, however, much in these reports which tend, I think, to give support to the creed.

I. Where do you find it?

E. Well, you will see that the Report of the Indian Com-

mission discredits both Contagion and Inheritance. Now, if you put these two possible causes aside, the disease must be either telluric or dietetic. It is not telluric, for it is the same in the most diverse regions, *ergo*, it is probably dietetic. Thus you will see we gain a step.

I. How about Dr. Newman's essay on "The Decline and Extinction of Leprosy in the British Islands"?

E. Dr. Newman supports the same conclusions, that the disease was not maintained either by inheritance or contagion. He shows conclusively that isolation was not the means which brought about the extinction of leprosy in Britain, but that it vanished before advancing civilisation. Now, as it is most certain that no amount of dirt, neglect or hardship, will of themselves produce leprosy, and conversely that it may prevail widely where these influences are wholly absent, we come again to the conclusion that it must acknowledge a food causation. Advancing civilisation means, in the main, improved food.

I. If, however, we accept the food hypothesis, it does not follow that dried fish is the one article to be suspected.

E. If you go into the facts you will soon find that such is the case. There is no other article of food the use of which is common to all leprosy districts, and to them only. I believe that I may say that the late Dr. Beaven Rake, from his work in India, and Dr. G. Newman from his investigation of historical facts in Britain, have both of them come very near to an acceptance of the salt-fish theory.

I. You cannot say that of Dr. Ehlers and his report on Leprosy in Iceland.*

E. No, I cannot. Dr. Ehlers seems to me to be a splendid example of that capacity for forgetting one class of facts whilst dealing with another, to which I just referred. His essay is a valuable one as far as history and narrative go, but his conclusions differ from mine, and as a consequence you will not expect me to compliment his powers of reasoning.

* Dr. Ehlers' prize Essay on Leprosy in Iceland can be had of Messrs. Macmillan. It will form part of a volume which will shortly be issued to members of the New Sydenham Society.

I. Will you allow me to read some passages from his work, and to hear your criticisms upon them?

E. With all my heart. It may be the best way of bringing out the nature of our differences.

I. Well, at page 22 I find, after a description of the hovels in which Icelanders live, the following:—

"One must have seen and smelt it all to be able to understand that the leprous disease is due neither to rancid butter nor to the dry fish. Just as well as rousing accusations against these articles of food which certain leprologists do, one might complain of the want of brooms to sweep the floor with, or insist that the leprosy is owing to the want of window-hooks, or to the stagnant water they drink. It is neither one nor the other of these moments, but the whole *ensemble;* it is the absolute want of cleanliness plus Armauer Hansen's bacillus, which in such an interior finds its true paradise."

E. Dr. Ehlers seems to forget that millions of the poor in the colder parts of the world live in close, dirty hovels not a wit better than those of the Icelanders, and yet get no leprosy, whilst the disease is extensively prevalent in some hot countries—India, Ceylon, the West Indian Islands, the Sandwich Islands—where the climate is delightful and where life is, in the main, in the open air. Further, in these countries not alone the poor, but members of the wealthy classes often suffer. He forgets that missionaries, officers in the English army, and others of the most careful personal cleanliness, going to live in India or Burmah, not unfrequently become lepers.

I. Are not you now forgetting that he does not accuse unhealthy dwellings alone, but associates also the bacillus?

E. No, I had not quite forgotten that all-potent bacillus, I was coming to that. There are a dozen good reasons for discrediting it as being the efficient cause of the spread of leprosy, all of which our author conveniently forgets. If the bacillus, plus dirty dwelling-places, could perpetuate leprosy, why did it die out amongst the English, Irish and Scotch peasantry? It had a wide prevalence once, and surely if these causes are sufficient it ought to be with us still. The bacillus was here, and the means of contagion were abundant,

and yet the disease ceased, just as it did also over almost the whole of Europe.

I. Dr. Ehlers would, I expect, say that the disease was exterminated by isolation.

E. He could allege that only by forgetfulness of all the facts as to the social history of the period. There was no efficient isolation. It is true that lepers in a loathsome stage of the disease were shunned. It is true also that leper houses were founded. There were then, however, neither the medical knowledge nor the social police by which alone efficient isolation could have been practised. Realise that leprosy is absolutely exterminated, and then imagine an attempt to get rid of tuberculosis by isolation. In many districts no attempts at isolation were made, and yet the disease ceased. In Iceland, which Dr. Ehlers has specially investigated, there were leper houses and a belief in contagion, but the disease has persisted. Contrast the Orkneys with Iceland. It has ceased in the Orkneys, and it still prevails in Iceland. Why? Let Dr. Ehlers supply the answer himself, for it is now my turn to quote:

"The essential nourishment of the Icelanders consists of *dried fish*, which is eaten cold with *rancid butter;* they even prefer the rancid butter that is kept for years, and to which the poor add a little (train) oil of whales or seals. The *sour milk* which they call "Skyr" forms a conspicuous part of their food, and can likewise be kept for years. *Bread is a rare* food, and does not form a prominent part, as Iceland does not produce any corn itself."

I. I must trouble you with a yet longer quotation. It is from page 23, and gives Dr. Ehlers' own comment on what you have just read to me:

"The adherents of the theory of the rancid butter and the rancid oil as the causes of the leprosy must not imagine that a house where they prefer or are content with the rancid butter is more careful with the other articles of food. The rancid butter or oil is only a symptom of the general filthiness which manures the soil in which the disease thrives. And then the poor fishes: the adherents of the theory of the fat fish will, no doubt, be content to hear that in Iceland it is especially the inhabitants of the coast who are affected with leprosy. But the explanation of this is quite simple, the interior of the island being almost

uninhabited and uninhabitable, and covered with glaciers. Also on Iceland they know the tradition about the leprous disease being due to far too exclusive an eating of fat fish. In Gullbringe it was the halibut, round the Lake of Thingvalla it was the trout which was accused of causing this horrible disease. It is a well-known fact that too monotonous a diet breaks down the resisting power of the organism as to diseases of every kind, but it can never create a specific and infectious disease. We also notice that the leprous disease has decreased considerably in the province of Gullbringe, and it has almost disappeared round the Lake of Thingvalla (I only found one single patient in this district), but it still teems with halibut and trout. All these theories may soon befittingly be buried beside that of Hjaltelin, who believed that the decrease of the leprous disease in Iceland was owing to the introduction of the potato. He considered the leprous disease as being founded upon the want of potassic salts in the blood. At the present time the potato has found its way to most of the Icelanders' tables, but unfortunately the leprous disease is on the increase. Is now this, may be, the potato's fault?"

E. These paragraphs contain much of interest. In the first place let us note that a suspicion as to dietetic causes has been prevalent in leprosy districts. This is true not only for Iceland but for all places where leprosy has prevailed. Now diseases which are spread only by contagion seldom come under a general popular suspicion as to food causation. Next we will note that fish is one of the articles which has been suspected all the leprosy-world over. As to the rancid butter and oil, I am not concerned to say anything further than that it is possible that they may be contributory causes. That they are not common causes of leprosy is obvious, for the disease is prevalent in countries where they are not taken. They may possibly be "manure to the soil," but they are probably not very active ones. The assertion that a too monotonous diet, nor in fact any kind of diet, "can never create a specific and infectious disease" may of course be accepted. I would ask, however, to what extent is leprosy "specific and infectious"? Not probably in any stronger sense than tuberculosis is so. The bacillus which attends it may be closely allied to that of tubercle, and under the laws of latency it may perhaps be present in most persons waiting only for the suitable "manure" to evoke its activity and give it temporary

specialisation. Dried fish may be not the infection conveyer, but only the stimulator. All this is for the present mere conjecture, but it has some plausibility.

When it is suggested that the greater prevalence of leprosy on the coast is to be explained solely by the want of inhabitants in the interior, Dr. Ehlers forgets that this coast distribution is not peculiar to Iceland, but occurs all over the world.* To the proposal that the theory that fish causes leprosy should perish with that of Hjaltelin that potatoes may have caused its decline, I say heartily Amen. The two may stand or fall together; they are parts of the same creed. The introduction of other kinds of food in substitution for fish has been probably, in the past, a very influential cause of the decline of leprosy. I do not, of course, accept Hjaltelin's opinion as to the potash salts, but this I do believe, that if we could give the poor Icelanders plenty of potatoes, bread, and fresh meat, raw dried fish would cease to be eaten, and leprosy would rapidly decline.

I. You will see that Ehlers in the last part of the paragraph assumes that leprosy is now on the increase in Iceland. Do you accept that, and, if so, how do you explain it?

E. I do not accept it. He has proved that there is more leprosy than had been presumed, but this is a very different thing from proving increase. The old statistics were worthless, as he admits, and more careful examination reveals the fact that there is more of the disease than had been supposed. This is what might have been expected. The general impression of the Icelanders themselves that the disease is declining is probably correct. The leper-houses are now disused, and history points strongly to the belief that the prevalence of the disease is very different now from what it was when its mere name was never mentioned without the pious ejaculation, "May God in His mercy save us." This, I believe, Dr. Ehlers would willingly admit, his

* Dr. Ehlers' suggestion that the advocates of the fish hypothesis will be "content to hear from him that leprosy is especially common on the coast" is perhaps a little unkind. Both the fact and its explanation were well known long before he wrote. An elementary knowledge of the topography of Iceland is not so exceptional as he seems to suppose.

assertions as to increase applying only to quite recent years.

I. You have not touched upon his arguments in support of the theory of infection. You will have seen that he begins his chapter on this topic—

"Is it really necessary in our day to heap proof upon proof of this simple fact?"

E. Yes, and what kind of proof does he heap up! He proceeds to cite the case of Father Damien and the inoculation of Kuanu, and follows them up by others, all of which, almost without an exception, are open to a source of fallacy which he appears to wholly forget.

I. What fallacy is that?

E. Why, this; that in every one of the cases which he cites to prove contagion, the disease may have been caused by food. The question in debate is whether leprosy is more probably induced by food or by contagion, and he considers that he proves his case by citing instances in which residents in leper districts, and consumers of what, for convenience of argument, I will call leprosic food, became lepers. They had, it is true, run risks of contagion, but they had also eaten the food, and no one can decide as to which influence proved potential. Father Damien went to live in a district where leprosy abounded; Kuanu, who became a leper after inoculation, was of a leprous family, he lived in a leprous district and ate lepers' food. His brother, who had not been inoculated, was a leper as well as himself. If heaping up "proof" of this kind satisfies Dr. Ehlers he may do it till he is tired, but he will never alter the opinion of any reasonable man. Mind, I do not deny the possibility of the contagion of leprosy. It may be, like that of tuberculosis, an occasional reality. I am, however, concerned to express in the strongest terms I can use, my utter incredulity as to contagion being an ordinary means of the spread of the disease.

Now see what Dr. Ehlers, in his advocacy of an exclusively contagious theory, forgets. He forgets that we receive lepers here in England every year, and allow them in all stages of

the disease to associate freely with others, and that no instances of contagion ever occur. He forgets that hundreds of Norwegian lepers have emigrated into the western states of America, and that without the observance of the slightest precaution (the dietetic cause being absent) they have most of them recovered, and that there has never been any spreading of the disease. He forgets that in our numerous Indian hospitals, where lepers are maintained, the nurses, dressers, and surgeons never contract the disease. He forgets that when Europeans who have lived in leprosy districts become lepers there is scarcely ever any history to be obtained of probable exposure to contagion. Finally he forgets that the disease has died out in England and in many other countries, although the opportunities for contagion continued to be abundant on all sides.

I. In accusing Dr. Ehlers of forgetting so much, are you not yourself forgetting an all-important fact which is recorded by many, that leprosy prevails in districts where fish is not eaten at all?

E. No; I have heard enough of that, and am never likely to forget it. It is, however, such a lengthy topic that I will, if you please, reserve it for another conversation. For the present I will simply say that I do not credit the statements.

No. CCVIII.—*Variolation and Vaccination.*

A CONVERSATION.

Ille. Is it really the fact that we were indebted to Lady Mary Wortley Montagu for the introduction of the practice of inoculation for small-pox?

Ego. Lady M. introduced it into London, and by her example and advocacy made it common in England, and afterwards in Christian Europe. It was not, however, really a novelty, for it had from remote times been practised in Wales and other places.

I. Is it, then, a mistake to suppose that it began with the Turks?

E. Yes; it was practised in so many and such distant parts of the world that we are driven to the belief that it was a device which had occurred independently to many minds as a means of contending against the demon small-pox.

I. Do you hold that there is an essential connection between small-pox and cow-pox?

E. Yes, certainly. It cannot now be doubted that Jenner and his friends were quite right in considering that cow-pox is variola in the cow.

I. On what evidence does that belief rest?

E. Over and over again the teats of cows have been inoculated with matter from human small-pox, vesicles have been produced, and the fluid from them used to vaccinate with.

I. What is Badcock's lymph?

E. Badcock was a chemist at Brighton who undertook variolation of cows on a large scale. He failed often, but he repeatedly succeeded, and he supplied lymph so obtained, from different cows and at different times, to the profession, and it was largely used, and is still so, for vaccination.

I. You say that others as well as Badcock have succeeded in this experiment.

E. Oh yes, many, although it must be admitted that many have failed. It is not easy to get the inoculation to take in the cow, but in France, Germany, India, and at home, those who have persevered have always at length succeeded.

I. Is there any danger that in taking lymph from a variolated cow you may convey small-pox?

E. Yes, there would appear to be a certain degree of risk. Mr. Ceely had an experience of this kind, but there are many fallacies which it is difficult wholly to exclude. We are hardly justified in believing that there is more risk of variolation in first vaccination of this kind straight from a variolated cow than there is after many removes.

I. You surely do not mean that there is risk of conveying small-pox in the ordinary methods of vaccination with lymph which has been passed repeatedly from calf to calf or from infant to infant?

E. I wish to assert clearly that vaccination is modified

variolation, and that the vaccinia which is produced is a modified variola.

I. But can vaccinia ever under the circumstances supposed be attended by the severe symptoms of small-pox?

E. I believe it never under any conditions becomes infective, and in this most important character it always differs from variola.

I. Can it, however, be attended by fever and eruption?

E. Certainly. In some cases a high degree of fever, with backache, &c., may be present, but no eruption; in others an eruption and but little fever, whilst in yet others both fever and eruption may be severe, and death may even result.

I. Is the eruption in these cases like that of small-pox?

E. Not usually. It may vary very much in character. Often it is only erythematous or lichenoid, and of very short duration. In severe cases, however, it may be indistinguishable from small-pox.*

I. It is, I believe, liable to produce gangrene?

E. It is, and to that condition the name "vaccinia gangrenosa" has been given. I have several portraits in the Museum which show it well.

I. Does the complication of gangrene depend upon debility in the child?

E. Not in the least. It may occur to strong, healthy infants, and is, so far as our knowledge extends, wholly a matter of idiosyncrasy.

I. You do not think that it depends upon any peculiar quality of the lymph employed?

E. No; for such cases are always isolated, they never occur in groups. Of ten infants vaccinated at the same time from the same source nine will do well and the tenth may have gangrenous vaccinia.

I. You do not recognise any contributory causes?

E. I know of none. So far as our knowledge at present goes it is a matter of accident.

I. But you hold, if I understand it, that there is con-

* The whole of this conversation was written before the occurrence of the case recorded at page 43.

stantly in vaccination a tendency to revert to its original character of variola?

E. Yes. I regard it as a fact strongly in favour of the protective power of vaccination, for it proves that the vaccinated do really pass through a modified form of variola.

I. Why was variolation, that is inoculation, for small-pox laid aside?

E. Because it was believed that by supplying foci of contagion it tended to increase the prevalence of small-pox.

I. Not on account of disappointment with its results?

E. By no means. On the contrary, it had, by careful attention to detail, been rendered very safe, and much enthusiasm was felt for it.

I. We may suspect that it was really the discovery of vaccination which brought it into discredit.

E. No doubt. It was not prohibited by law until long after vaccination had been fully established. Very curious bits of evidence as to the terror of small-pox and the confidence which was placed in inoculation may be found in the older writers. Adams records that a country clergyman had expressed fears that all country people would flock to London now that inoculation had made the metropolis safe.

I. Was, then, the fear of small-pox a real reason for avoiding London?

E. So Adams and others tell us. Small-pox was always present in London, whilst in many country towns and most villages it was unknown. Many country residents visiting London caught small-pox, and a considerable number died. London was avoided then much as it would be now if an epidemic were raging in it.

I. Was it, then, the fact that most Londoners suffered from it?

E. Adams tells us that it was exceptional for a person to have reached the marriageable age without having had small-pox, and he relates, as very extraordinary, the case of a young lady who had been brought up in London and who contracted small-pox during a visit to the country.

I. In what year did Adams write?

E. Dr. Joseph Adams, the pupil and friend and exponent

of John Hunter, wrote in the end of the last century and beginning of this. His work on Morbid Poisons, from which I have quoted, was published in 1806, but a first edition had appeared ten years earlier. He was physician to the Small-pox and Inoculation Hospitals.

I. Was he a convert to vaccination, as he had had opportunities for observing the results of it as well as of inoculation?

E. He wrote in 1810, that is about ten years after Jenner's first publications, "I should conceive it lost time to offer even a summary of the arguments adduced to prove that cow-pox is a security against small-pox. There is, in my opinion, no medical fact which stands on a securer foundation."

PLATES CXV. & CXVI.

DUPLICATION OF THE SKULL. CRANIOPAGUS.

THESE Plates are taken, the one from a specimen in the Museum of the Royal College of Surgeons, and the other copied from a Plate published by John Hunter in the Transactions of the Society for the Improvement of Medical Science. The Plate CXV. shows the condition during life of the child whose skulls are depicted in Plate CXVI. The child was born in India, and lived to the age of four years, when it was killed by the bite of a rattlesnake. As will be seen, it possessed two heads united by their crowns. The face of each was complete, and there were no other abnormalities in the development of the body or limbs. The duplicate head possessed the power of moving its eyes, cheeks, and lips. There was no neck. The base of the skull shows the foramen magnum and the other foramina considerably smaller than those of its fellow skull. The lower jaw was also less well-developed. The specimen is described by Mr. Lowne in the catalogue of the Teratological Collection of the College, page 37, No. 138. It will be seen that the faces are not directed in the same line. The left frontal bones of one skull articulate by sutures with the right frontal bones of the other, and *vice versa*. The external ears were represented by mere folds of skin, and there was no auditory meatus. The brains were distinct, each invested in its own membranes, which adhered at the points of contact. It is stated that the movements of the face of the accessory head were not controlled by the feelings or desires of the child, and that the eyelids were usually open, even during sleep.

West Newman lith.

West Newman lith

ARCHIVES OF SURGERY.

APRIL, 1896.

ON VARIOLATION FOR THE PREVENTION OF NATURAL SMALL-POX.

At the present juncture, when a not inconsiderable section of the community is desirous to abandon the benefits of vaccination, it becomes a matter of much interest to remind ourselves of what was the condition of things in pre-vaccination times. As contributory to this object, I have thought it worth while to reprint a letter of Voltaire's which bears contemporary testimony to the opinions extant at the date of the introduction of the practice of inoculation into England. At the time that it was written, as will be seen, this practice had not spread to France. Those of my readers who may not have been already familiar with Voltaire's letters may perhaps feel grateful for having their attention called to these witty and well-informed compositions.

My principal object is to reproduce the letter, but I will append to it a few comments upon it and upon the practice of inoculation.

VOLTAIRE ON INOCULATION.*

"It is inadvertently affirmed in the Christian countries of Europe that the English are fools and madmen. Fools,

* The following letter by M. de Voltaire is taken verbatim from a translation of that author's "Letters concerning the English Nation" published in Glasgow in 1752. The original letter was probably written between the years 1728 and 1731. These were the years of Voltaire's residence in England.

because they give their children the small-pox to prevent their catching it; and madmen, because they wantonly communicate a certain and dreadful distemper to their children, merely to prevent an uncertain evil. The English, on the other side, call the rest of the Europeans cowardly and unnatural. Cowardly, because they are afraid of putting their children to a little pain; unnatural, because they expose them to die one time or other of the small-pox. But that the reader may be able to judge whether the English, or those who differ from them in opinion, are in the right, here follows the history of the famed inoculation which is mentioned with so much dread in France.

"The Circassian women have, from time immemorial, communicated the small-pox to their children, when not above six months old, by making an incision in the arm; and by putting into this incision a pustle, taken carefully from the body of another child. This pustle produces the same effect in the arm it is laid in, as yeast in a piece of dough: it ferments and diffuses through the whole mass of blood the qualities with which it is impregnated. The pustles of the child in whom the artificial small-pox has been thus inoculated are employed to communicate the same distemper to others. There is an almost perpetual circulation of it in Circassia; and when, unhappily, the small-pox has quite left the country, the inhabitants of it are in as great trouble and perplexity as other nations when their harvest has fallen short.

"The circumstance that introduced a custom in Circassia, which appears so singular to others, is nevertheless a cause common to all nations, I mean maternal tenderness and interest.

"The Circassians are poor and their daughters are beautiful; and indeed it is in them they chiefly trade. They furnish with beauties the seraglios of the Turkish Sultan, of the Persian sophy, and of all those who are wealthy enough to purchase and maintain such precious merchandise.

"A trading nation is always watchful over its own interests, and grasps at every discovery that may be of advantage to its commerce. The Circassians observed that scarce

one person in a thousand was ever attacked a second time by a small-pox of a violent kind. That some, indeed, had this distemper very favourably three or four times, but never twice so as to prove fatal; in a word, that no one ever had it in a violent degree twice in his life. They observed farther, that when the small-pox is of the milder sort and the pustles have only a tender, delicate skin to break through, they never leave the least scar in the face. From these natural observations they concluded that in case an infant of six months or a year old should have a milder sort of small-pox, he would not die of it, would not be marked, nor be ever afflicted with it again.

"In order, therefore, to preserve the life and beauty of their children, the only thing remaining was to give them the small-pox in their infant years. This they did by inoculating in the body of a child a pustle taken from the most regular and at the same time the most favourable sort of small-pox that could be procured.

"The experiment could not possibly fail. The Turks, who are people of good sense, soon adopted this custom, insomuch that at this time there is not a bassa in Constantinople but communicates the small-pox to his children of both sexes immediately upon their being weaned.

"Some pretend that the Circassians borrowed this custom anciently from the Arabians; but we shall leave the clearing up of this point of history to some learned Benedictine, who will not fail to compile a great many folios on this subject, with the several proofs or authorities. All I have to say upon it is that in the beginning of the reign of King George the First, the Lady Wortley Mountague, a woman of as fine a genius and endued with as great a strength of mind as any of her sex in the British kingdoms, being with her husband, who was ambassador at the Porte, made no scruple to communicate the small-pox to an infant of which she was delivered in Constantinople. The chaplain represented to his lady, but to no purpose, that this was an unchristian operation, and therefore that it could succeed with none but infidels. However, it had the most happy effect upon the son of the Lady Wortley Mountague, who, at her

return to England, communicated the experiment to the Princess of Wales, now Queen of England. It must be confessed that this Princess, abstracted from her crown and titles, was born to encourage the whole circle of arts and to do good to mankind. She appears as an amiable philosopher on the throne, having never let slip one opportunity of improving the great talents she received from nature, nor of exerting her beneficence. It is she who, being informed that a daughter of Milton was living, but in miserable circumstances, immediately sent her a considerable present. It is she who protects the learned Father Courayer. It is she who condescended to attempt a reconciliation between Dr. Clark and Mr. Leibnitz. The moment this Princess heard of inoculation she caused an experiment of it to be made on four criminals sentenced to die, and by that means preserved their lives doubly: for she not only saved them from the gallows, but, by means of this artificial small-pox, prevented their ever having that distemper in a natural way, with which they would very probably have been attacked one time or other, and might have died of in a more advanced age.

"The Princess, being assured of the usefulness of this operation, caused her own children to be inoculated. A great part of the kingdom followed her example, and since that time ten thousand children, at least, of persons of condition, owe in this manner their lives to Her Majesty, and to the Lady Wortley Mountague; and as many of the fair sex are obliged to them for their beauty.

"Upon a general calculation, threescore persons in every hundred have the small-pox. Of these threescore, twenty die of it in the most favourable season of life, and as many more wear the disagreeable remains of it in their faces so long as they live. Thus a fifth part of mankind either die or are disfigured by this distemper. But it does not prove fatal to so much as one among those who are inoculated in Turkey or in England, unless the patient be infirm or would have died had not the experiment been made upon him. Besides, no one is disfigured, no one has the small-pox a second time, if the inoculation was perfect. It is therefore

certain that had the lady of some French ambassador brought the secret from Constantinople to Paris, the nation would have been for ever obliged to her. Then the Duke de Villequier, father to the Duke d'Aumont, who enjoys the most vigorous constitution and is the healthiest man in France, would not have been cut off in the flower of his age. The Prince of Soubise, happy in the finest flush of health, would not have been snatched away at five-and-twenty; nor the dauphin, grandfather to Lewis the Fifteenth, have been laid in his grave in his fiftieth year. Twenty thousand persons whom the small-pox swept away at Paris in 1723 would have been alive at this time. But are not the French fond of life, and is beauty so inconsiderable an advantage as to be disregarded by the ladies? It must be confessed that we are an odd kind of people. Perhaps our nation will imitate, ten years hence, this practice of the English, if the clergy and the physicians will but give them leave to do it: or possibly our countrymen may introduce inoculation three months hence in France out of mere whim, in case the English should discontinue it through fickleness."

Here ends this remarkable letter.

Amongst the points in it to which attention may be asked are the following:—

1. The full recognition that small-pox may occur more than once, or even three or four times, to the same patient.

2. That second attacks are always milder than first ones and never fatal.

3. That small-pox produced by inoculation was reputed to leave no conspicuous scars.

4. That the fatality of inoculated small-pox was by general repute very low.

5. The estimate given by Voltaire of the fatality of natural small-pox in his day. He believed that sixty out of every hundred caught the disease, and that one-third of them died.

6. Voltaire was aware that the practice was not confined to Turkey. His prediction that some "learned Benedictine" would write folios on its history has been fulfilled.

Inoculation was, as is well known, abandoned in England, and finally made unlawful, because it was believed to be a cause of the spread of small-pox. It furnished fresh foci for its spread by infection. This objection would, however, no doubt have been overcome, by systematic isolation of those inoculated, had it not been that the discovery of vaccination rendered the practice no longer needful. Not all the "fickleness of the English" would have availed to put inoculation out of use had it not been that a better alternative had been found.

Some of the more intelligent of those who now oppose vaccination on the plea of inefficiency are compelled to hark back on inoculation as its substitute. One of the most noteworthy answers recorded in the volumes of evidence already published by the Vaccination Commission (Fourth Report, p. 121) is the reply of Professor Crookshank to a question on this point.

ON THE NATURE OF YAWS.

(*Continued from p.* 82.)

In the last number of my ARCHIVES I endeavoured to introduce the consideration of the true nature of Yaws by giving some pictorial illustrations of typical forms of the disease, and also some fragmentary notices of it from former writers. My contention is clear and definite to the effect that there is no such disease as Yaws, and that the phenomena which have received that name are simply those of syphilis modified by race, climate, and by erratic modes of infection. Although I state this definitely as my own firm belief, yet I trust that I entertain due respect for the opinions of those who differ from me, and fully admit that the subject is one of much difficulty.

I have already endeavoured to show that Frambœsoid eruptions are by no means unknown in European syphilis, and that it would appear that they have been especially common in certain neglected cases in which the disease has been communicated by non-venereal intercourse. Thus it has been shown that the Sivvens of Scotland and the Frambœsia Cromwelliana described by Dr. Wright were nothing but syphilis. I have also already shown that those who have written upon Yaws are by no means agreed as to the most important facts. On this point I shall yet have much to offer. The circumstance upon which one writer relies for its differentiation from syphilis may be found to be utterly denied as a matter of fact by another writer of equal experience and authority. I now reprint a tolerably full statement of the question which I wrote three years ago as a preface to an Essay on Yaws by Dr. Numa Rat, of the Leeward Islands. Dr. Rat's Essay, with my preface, was published as a Government Blue Book, but has probably found its way into the hands of but very few. Since it was written another Blue Book, which is in some ways an answer to it, has been published from the able pen of Dr.

Alford Nichols. This work, and also many other communications in print and in manuscript on this subject, I have carefully read. The result has been that the opinions hinted at in the following paper have been much strengthened. I reprint the paper, however, with but few alterations. It will, I trust, place the whole question clearly before the reader. On another occasion I shall examine Dr. Nichols' arguments and those of others who still hold that Yaws is distinct from syphilis. This will be my summary, and will conclude my references to the subject. I shall, during the three months which will elapse before its publication, feel much indebted to any one who can supply me with new facts or criticisms. I may add that Dr. Nichols' Report contains numerous excellent pictorial illustrations of the disease, which should be carefully examined by any one interested in the subject. These, at the time when I first wrote, were a desideratum.*

ON THE NATURE OF YAWS.

The decision as to the true nature of the locally restricted disease known as Yaws must be recognised as being one which is still surrounded by some doubt and difficulty. All the more recent observers who have studied the malady in its native haunts are, I think, unanimous in the opinion that it is not syphilis. The confidence with which this opinion is held depends, however, to a very large extent upon impressions received by actual observation rather than upon facts which can be stated in print and submitted to the judgment of the reader. Our author (Dr. Rat) describes it as a disease which is communicated by contact, and which acknowledges a primary sore. After a definite interval a febrile stage follows, attended by an erythematous or scaly eruption, after which come the more typical appearances in the form of scattered fungating ulcers. Finally, after variable periods of months or years, we have in many instances tertiary phenomena in the form of "gummata." To these facts, as to its natural course, we have to add that mercury and the iodides are specific remedies for the disease

* Dr. Nichols' Report is in most of our London medical libraries.

in all its later stages, and that for a permanent cure mercury is more to be trusted than the iodides. If, then, Yaws be not syphilis, it is clear that it offers a very exact parallel to it. They are diseases as like each other as are true measles and German measles. Indeed, in the latter instance we are not able to clench the statement of similarity by adducing proof of curability by the same specifics, as can be done in the case of Yaws and Syphilis.

Our first duty after what has been said must be to examine the evidence which is relied upon when it is asserted that Yaws is not really a form of syphilis. Upon this point it must be admitted that the expressions of writers are not quite in unison. I have carefully compared the excellent essay on Yaws which is supplied by Dr. Hirsch * with that which is now before me. In the former, one of the strongest arguments as to its difference from syphilis is that Yaws is a local rather than a constitutional disease, and above all, that it is spontaneously curable, and leaves no liability to subsequent or tertiary phenomena. Dr. Rat, however, makes no doubt whatever that it is a constitutional and febrile malady, and gives us case after case in which tertiary gummata were developed after periods of several years. Thus, in Case VII., a boy had Yaws in childhood and was the subject of gummata at the age of twenty-five. Cases V. and VIII. are very similar ones.

If, then, we take the facts as stated in the present Memoir as being the most recent, and as at least equal in accuracy to any preceding account of "Yaws," we have to understand by that name a disease which begins by a local sore, becomes constitutional, observes stages, and has both secondary and tertiary phenomena. Having assumed these facts as established, we must return to the fundamental question: Can it be proved that this malady is not, after all, syphilis, modified by race and climate? I will endeavour to state, in the first place, the arguments which seem to imply probability that it is a form of syphilis, and afterwards those which bear in the opposite direction.

* See "Hirsch's Handbook of Historical and Geographical Pathology," published in translation by the New Sydenham Society, vol. ii. p. 101.

A SCHEDULE OF THE STAGES OF YAWS DURING THE FIRST YEAR.

(*Compiled chiefly from* Dr. Numa Rat's *Report.*)

1st month.	An *Incubation Stage* of from three to ten weeks. No symptoms. Ulcers, excoriations and wounds are believed to facilitate the implantation of the virus.
2nd month.	A primary lesion, which consists of a papule, which at the end of about seven days develops a pale yellow fluid at its apex. A scale is formed, and an ulcer results, with perpendicular edges and a clean base. The ulcer usually heals of itself in about a fortnight, but may last longer (p. 8). The lips, the breast, the groin, perineum and genitals are the parts upon which primary sores most frequently occur. There is often considerable fever and severe pains in muscles, joints, &c. The fever is worse at night.
3rd month.	As a rule, the primary lesion disappears before the secondary phenomena are observed. An eruption of little red spots covers the patient from head to foot ("like lichen tropicus"). It develops in a few days and begins to fade within a week. A few of the spots, however, do not disappear, but develop into papules (p. 9) which pass into tubercles, and finally present papillary fungations like cauliflower buds. Non-suppurative periostitis may occur. There is no enlargement of lymphatic glands.
4th month.	The tubercles rapidly become paler and shrink until level with the skin. They may vary in number to any extent, there being sometimes only one or two. Several may coalesce and form a large patch of granulation tissue. They frequently unite to form rings round the eyes, nose, mouth, or anus. In some conditions they resemble "small carbuncles," and in others "the mucous patches of syphilis." The palms and the soles are often affected, and in a peculiar manner (p. 11).
5th month.	"The normal end of the tubercle is its disappearance by interstitial absorption at the end of about six weeks from the time of its development" (p. 11). [Treatment by mercury ought to have been commenced, according to Dr. Rat's opinion, as soon as the tubercles were mature, and this practice may possibly have modified his estimate of the duration of the secondary eruptions.]
6th month.	Although in most cases the eruption has disappeared spontaneously by the end of the fourth month, yet in many (especially perhaps in feeble and under-fed patients) it may have persisted and become aggravated.
7th month.	In most cases the patient is well, but in some the eruption persists.
8th month.	In most cases the patient is well, but in some the eruption persists.
9th month.	In most cases the patient is well, but in some the eruption persists.
10th month.	"In unhealthy and ill-fed persons, *without treatment*, the average duration of the eruption is nine months," but "tubercles may continue, or may disappear and be replaced by others, during several years" (p. 11).
11th month.	In exceptional cases there may still be patches of fungus growth, and great debility.
12th month.	It is only in exceptional cases that symptoms persist. For these treatment by mercury is essential.

NOTES ON THE TERTIARY PERIOD OF YAWS.

It would appear that in a majority of cases in which symptoms ranked as tertiary occur they are continuous with those of the secondary stage, the patient having never been free. In others, however, they supervene after a longer or shorter period of health. In the former case they consist chiefly of ulcers extending serpiginously and more or less deeply, and in the latter they begin as subcutaneous swellings, which are classed as "gummata." The leg is the part most frequently affected. Destructive ulceration of the nares, pharynx, and soft palate is also one of the later manifestations of Yaws. It may occur twenty years after the last traces of secondary symptoms. Chronic periostitis may occur. There may be exfoliation of the smaller bones of the hands and feet. In severe cases, especially when the throat is ulcerated, or obstructed by growths of granulation tissue, there may be cachexia, but "it is surprising to what an extent tertiary lesions may exist without seriously impairing the health."

These tertiary symptoms are, according to our author, curable only by the judicious use of specifics (mercury and iodides), with the local application of iodoform, &c.

Facts in favour of Identity of Yaws with Syphilis.

In order to bring out the facts more clearly, I have drawn up from the data supplied by Dr. Rat, in many respects confirmed by others, the preceding schedule of the stages of Yaws.

I will now proceed to ask attention to the facts which appear to favour the opinion that syphilis and Yaws are one and the same disease.

1st. *The stages observed by Yaws are very similar to those of syphilis.*—It would appear that in many cases the initial lesion is not easily identified, and in these, of course, the length of the incubation period cannot be accurately estimated. So far as the facts go, however, it would seem probable that about a month elapses between the reception of the virus and the development of phenomena in the part inoculated. The stage of fever, osteocopic pains and eruption, follows in a few weeks. These are almost exactly the stage-periods of syphilis. The development of fungating rupial sores, which follow what we may call the exanthem eruption, occurs also just at the time when in syphilis the first eruption may slide, under ineffectual or prejudicial treatment, into rupia. (An exceptional but still a well-recognised event.)

2nd.—*The tertiary symptoms of Yaws very closely resemble those of syphilis, and occur after intervals of much the same irregularity of duration which we notice in the case of syphilis.*—Thus in some instances they begin as soon as the secondary stage is well over, and in others they are delayed through many years of latency. Gummata in the cellular tissue and ulcerative destruction of the palate are precisely the occurrences most common in similar stages of syphilis.

3rd. *The methods of cure as regards the constitutional and later stages of Yaws are precisely those adapted for syphilis.*—As I have already pointed out, the strongest argument against the identity of the two diseases with former writers was the spontaneous curability of Yaws. We were told that without treatment it always got well, and that it had no sequelæ. This argument is, however, wholly overthrown by Dr. Rat, who not only describes the tertiary symptoms, but

alleges that they may last indefinitely unless they are cured by the very drugs which are found to be specifics for syphilis. The judicious rules as to treatment which he lays down are precisely those which many would propound as regards English syphilis. He is not a strong mercurialist, and has no notion of an abortive or early treatment, believing rather that the first stages are made worse by mercury, and reserving the remedy for the later ones. We must remember, however, that these are precisely the doctrines which until recently were extensively held as regards the use of the drug in syphilis. It may easily be the fact that when an abortive plan by small doses is carefully tried in Yaws it will be found to be as effectual in preventing the phenomena of that malady as it is in respect to those of syphilis.

4TH. *The character of the secondary eruption would appear to vary much as in syphilis.*—Thus, while Dr. Rat calls it "erythematous," I find other writers speaking of it as "scaly" and "papular" &c. (see Hirsch, p. 102).

5TH. *Its restriction to certain localities.*—Another argument in opposition to the idea that Yaws depends upon a specific virus distinct from that of syphilis may be based upon the fact that the disease (or perhaps we should rather say its peculiarities) appears to be restricted to certain races and regions. No Englishman comes back to us the subject of Yaws. It would appear that it is a malady which cannot leave its home. Now it is *à priori* far more likely that race and climate should be able to stamp such a disease as syphilis with peculiarity than that they should be able to confine to themselves the operation of a powerful specific poison. We have, so far as I know, no other instance of a specific animal poison which is restricted in its operation by race and climate.

Papillary excrescences are known also in syphilis.—The tendency to papillary outgrowth and the production of fungating excrescences which is the most obvious peculiarity of Yaws is by no means wholly absent in syphilis as seen in European practice. I have myself repeatedly asked attention to this tendency to outgrowth as sometimes seen on the

tongue in the secondary stage of syphilis, and syphilitic warts and condylomata (a form of papillary outgrowth) have long been well known. The tendency to papillary outgrowth appears to be a peculiarity of the individual who is under the influence of disease, and may easily be an appanage of race. The skin in the negro and other dark races, as is well known, has a large papillary development. Pruner has already suggested that this may explain the peculiarity of Yaws.

Reasons for believing that Yaws is distinct from Syphilis.

The unanimity of opinion on the part of local observers.—We must certainly place first among the reasons for believing that Yaws and syphilis are distinct maladies the conviction expressed by those who, like Dr. Numa Rat, have had abundant opportunities for observing both.* It is remarkable that almost all local observers assert non-identity, whilst most European pathologists, who have before them only printed evidence, incline to believe in essential identity. Amongst the latter, and with Copland and Lancereaux, I should, without any misgiving, rank myself, were it not for the personal convictions of men like Dr. Rat, who have had opportunities for seeing and comparing the two diseases which have not fallen to my lot.

That it is a disease that usually begins in childhood.—It is admitted that it may be contracted at any age, but, whilst in Europe syphilis is most frequently contracted by adults, Yaws, amongst the people liable to it, is most frequently acquired by children. Now, it is to be admitted that children are just as liable to contract syphilis as adults, and in order to estimate the value of the argument we require to know the social differences of the two communities. In Europe the cleanliness and care with which children are as a rule tended prevents their contracting syphilis in any large

* Dr. Hirsch writes, "In the opinion of nearly all observers, Yaws is *a peculiar and specific infectious morbid process, a disease* sui generis, *which has nothing whatever in connection with syphilis.*" New Sydenham Society's Translation, p. 103. (The italics are as in the original.)

numbers. A few do by accident become infected, and it is probable that those who thus suffer in childhood are far less liable than others to contract the disease in adult life. Thus if it should chance to be the fact that amongst the poorer Africans of the districts where Yaws occurs the precautions against accidental inoculation of a disease such as syphilis are less well observed, it might easily follow that more would have it in childhood and fewer in adult life. On this point we need more precise information than we possess as to the conditions under which young children became the subjects of Yaws. Is it usual to be able to trace the source and mode of contagion? What is the character of the initial lesion?

That it is not transmitted by inheritance.—More evidence is required on this point, and I have already suggested some fallacies.*

That some of the phenomena which occur in syphilis are not met with in Yaws.—Amongst these Dr. Rat enumerates lesions of the mucous membranes in the early stage, alopecia, affections of the eye, orchitis, and paralysis of ocular muscles.

* At p. 62 Dr. Rat confutes the statement of previous observers that Yaws is common in young infants. His statistics, and they are supported by those given by Nicholls and Tulloch, show that the period most liable is that between five and ten, and next that between ten and twenty, and that the first quinquennium has a much smaller proportion. Tables C, D, E and F, pp. 60 and 61, must be held to prove the rarity of the disease in the first year of life, and at the same time that it appears to make but little difference whether or not the parents have had the disease. We must admit that there is no proof of any inherited form. There is, however, again a possible fallacy which must be taken note of. Syphilis is usually inherited from parents who have quite recently had the disease, and we are accustomed for practical purposes to limit the period of possible transmission to two years. If, then, Yaws be syphilis, and if in a Yaws community nearly 60 per cent. of the children (see p. 61) acquire the disease in the first quinquennium, and are exempt afterwards, then it is clear that we ought not to expect hereditary transmission as a frequent occurrence. In saying this, I most freely admit that there ought to be some tolerably definite examples of inheritance every now and then. Cases must occur in which individuals become parents soon after the acquisition of Yaws, or even whilst the secondary symptoms are extant. A collection of half a dozen such, with the definite statement that the infants born under such conditions remained healthy during the first year of life, would go far to sustain scepticism as to hereditary transmission.

It must be remembered, however, that some of these are not common in syphilis, and that it is very possible that more extensive observation may yet discover them in association with what is called Yaws. Race may also have something to say as to their supposed absence. They are not conditions upon the absence of which much stress can be placed.

That it is very rarely attributed to sexual intercourse.—The primary lesion is sometimes, but not frequently, met with on the genitals. Its more common sites are the lips, the breast, or the limbs. The reply to this argument is that, inasmuch as Yaws usually follows accidental infection in childhood (50 per cent. of the children suffering in some districts), we ought not to expect infection at that age to occur on the sexual parts. If it be syphilis, it is clearly for the most part syphilis *sine coitu.* The following statements read very suspiciously:—

"When a papule develops on the lip, neck, shoulders, or breast of a woman who is nursing a child suffering from Yaws, or on a child who is being nursed by an infected mother, or when the same appears on the lips or genitals of a person cohabiting with another who is affected with the disease, and such a papule proceeds to ulcerate, there are sufficient grounds for assuming the probability of infection, and for consequently isolating the person in whom such a lesion presents itself" (p. 49). These are obviously precisely the expressions which would be applicable to the primary lesion of syphilis.

The primary sore is never an induration.—Here again we must remember that the primary sore occurs erratically, and on parts other than the genitals, and that many, or even most, of the erratic chancres of true syphilis are also destitute of characteristic hardness. It is on the genitals almost solely that the more characteristic chancres are encountered. If, in the event of a Yaws sore occurring on the genitals, it were at the same time attended by induration, it would, we may fairly suggest, be probably diagnosed as syphilis, and not Yaws.

That it is not attended by characteristic enlargements of

the lymphatic glands.—It is not asserted that the glandular system wholly escapes, but that it suffers in much less degree than in syphilis, and none of the adenopathies are what is called characteristic. Dr. Rat's words are (p. 33), "There are no alterations in the structure or function of these organs, which can be considered as the special result of the Yaws poison." He fully admits that the glands may enlarge in connection with ulcerations, and says that they may present, under such circumstances, "a comparatively soft unyielding painless mass, without any tendency to inflammation, induration or suppuration." Such expressions as these diminish considerably the importance of the asserted distinction between Yaws and syphilis, that the one has enlarged glands and the other not. With the exception of the indurated glands which constitute the primary bubo, the implication of the lymphatic system in syphilis is probably far less definite and less characteristic than is generally supposed. Even the bubo is often omitted, so that here again the difference between the two appears to be, after all, only a question of degree.

That it is much more readily susceptible of spontaneous cure than is syphilis.—The force of this statement, which, in the pages of Hirsch and others, seems almost conclusive, is very greatly reduced by Dr. Rat's report. This latter, although it by no means makes Yaws as severe a disease as syphilis, yet clearly establishes the fact that the differences are only in degree. It asserts strongly that some forms of Yaws are incurable, excepting by the use of specifics.

Concerning the relations of some other Diseases to Yaws and to Syphilis.

At p. 18, Dr. Rat deals with certain other diseases which it is necessary to diagnose from Yaws. Of Pian, a disease of the French West Indian colonies, he writes: "It is evident from this account that it is the same disease as Yaws." The account referred to is: "Pain in the limbs with fever, red spots on the skin, which become scaly, an eruption consisting of fungous excrescences of varying size

and colour, some being as large as the hand and discharging a thick sanious matter. The later lesions are excrescences on the soles and palms, from which a purulent secretion escapes, or inflammation and consequent thickening of these parts; pains in the bones, followed by enlargement or softening of the same; ulcers over various parts of the body; destructive ulceration of the bones, of the face, palate," &c. It will occur to most readers of such a description that, if it proves the disease so described to be Yaws, it raises, at the same time, a strong suspicion that both are syphilis.

The Peruvian Verrugas, on evidence which no one will doubt, is asserted to be wholly distinct from Yaws. There can be no doubt that Verrugas is a disease which attacks newcomers and foreigners more readily than natives. It is strictly endemic, but may be brought away by a European returning home.* It has no initial lesion or primary sore, nor does it display stages. It is in all probability due either to some poison taken in the food or to some parasite. Its only feature of similarity to Yaws is that both produce fungating or papillary outgrowths.

Concerning the nature of the Button Scurvy of Ireland, Parangi of Ceylon, the Delhi boil, Sibbens, Radesyge, Mal de Chicot, and Mal de Scherlievo, our author has nothing to tell us from his own knowledge, but quotes from Hirsch, and other authorities, such statements as best assist in the comparison of them with Yaws. Premising the caution that we must not trust too much to reputed facts, and reminding Dr. Rat that he has himself, in regard to Yaws, confuted several statements of his predecessors, it may be remarked respecting this group of endemic maladies, all more or less resembling both syphilis and Yaws, that its mere existence favours the suspicion that syphilis may receive modifications in connection with race and social habits. The Delhi boil we may dismiss, as being wholly different, whilst of the Radesyge, Sibbens, Mal de Chicot and Mal di Scherlievo, we

* Of this witness the case of the man, a patient of my own, who was the subject of the beautiful plate illustrating Verrugas, published by the New Sydenham Society.—Plate XLI.

may say that there is but little doubt that these names were given to true syphilis. Concerning Parangi there is perhaps more doubt, so good an authority as Dr. Kynsey holding, from personal observation, that the two diseases are distinct. Yet in each and all of these affections there are features remarkably similar to those of Yaws. All are said to occur chiefly amongst the poor and neglected, and in all a tendency to overgrowth of papillæ is witnessed.

Does there exist a family of Syphiloids?

The question remains to be asked as to whether there exists a family of diseases allied to syphilis but not identical with it. Dr. Rat is himself quite willing to entertain the question whether Yaws may not be the parent form of what we now know as syphilis, and suggests that some, at least, of the diseases just named belong to a "syphiloid group." In examining such a question as this we must be very careful to define our terms, or we may easily add to the confusion of a perplexing subject. Do we mean by "syphiloids" diseases which have the same laws of development as syphilis itself but are really quite distinct from it, or do we mean modifications of one and the same malady brought about by such influences as race, climate, and social habits? It is quite certain that in the same community syphilis does receive modifications from the peculiarities of constitution of those in whom it occurs. It is easily conceivable that transmitted from person to person, through many generations in one and the same race (the Cingalese or the Negro, for instance), the disease might acquire some minor persisting features of difference. It is possible that Parangi in Ceylon, and Yaws in Africa may be syphilis so modified. We should be going, however, much further than any facts in our possession warrant if we were to suppose that syphilis so modified could become a disease capable of existing side by side with its progenitor in an independent position. If Yaws be only syphilis modified by race, or syphilis only Yaws under the influence of European civilisation, then we should certainly not expect that the two could co-exist. This last

fact is, however, the assertion of local observers. Dr. Rat distinctly asserts in his synopsis of "General Differences," on p. 23, that "syphilis may be contracted by those suffering from Yaws," and that conversely "Yaws may be contracted by those suffering from syphilis." If these statements are to be taken in their full apparent significance, if we are to understand by them that syphilis runs its usual course in a patient who has recently suffered from Yaws and *vice-versâ*, then we have clinical proof that the two have attained specific distinctness, and at the same time most will, I expect, be willing to admit that it becomes extremely improbable that the two can have ever, within historic periods, been other than specifically distinct.

In order to arrive at a satisfactory solution of this and other cognate questions we need more detailed information as to the peculiarities which syphilis displays in relation to race, and more especially in those countries where these "syphiloid" maladies prevail. It is the knowledge that local residents possess this information which induces us to attach so much importance to their opinions when they assert, in contradiction to the general bearing of the evidence, that the two maladies are distinct.

If, however, we are to accept in Yaws and Parangi diseases related to syphilis, but which have acquired the position of species, and may now be propagated independently of it, then it would certainly follow that we should expect that they would no longer be restricted to race or to locality. Yet nothing that is well marked in the form of Yaws finds its way to Europe, and in the countries where it prevails Europeans appear to escape almost wholly.

MORULA: WHAT WAS IT?

MR. WILLIAM WALLACE was, during the early part of the present century, surgeon to the "Infirmary for the Treatment of Diseases of the Skin in Dublin." In December, 1826, he communicated, through Mr. Travers, to the Medico-Chirurgical Society a paper entitled "History of a Fungous Eruption curable by Mercury, but not of Venereal Origin." This paper appears to have been, with many others, forgotten or wholly neglected by subsequent writers, but it is most desirable that we should renew acquaintance with Mr. Wallace's statements. He appears to have been an observant man, and to have had large experience in diseases of the skin. He speaks of having seen at his hospital fifty examples of the disease which he described. Between 1818 and 1826 eighty-one cases had been treated at the Hospital. Of these he gives three in detail. He proposes the name "Morula, from *morus*, a mulberry," for the malady; which had, he thought, not been previously described. He mentions its resemblance to the diseases described as Yaws, of which latter he had no personal knowledge.

The three cases which Mr. Wallace describes all had men of the labouring class for their subjects. In one the man was forty, in another twenty-three, and in the third nineteen. In two the eruption had been present four months, and in one three. In all it was quickly and completely cured by mercury pushed to slight ptyalism, and the author was strongly of opinion that no other treatment would cure it. He says that he had never seen the eruption in very young or very old subjects, but in an appended note records as an exception the case of an infant who contracted the disease from its nurse, the latter not being its mother.

The prominent feature of the eruption was a fungus of a rounded or granulated form, often occurring on the hairy parts and glueing the hairs together, but not causing them to fall out. Often these excrescences were covered by a pus-crust which could be detached without causing bleeding. A second stage of the fungus was in many cases an ulcer, and when this happened a scar was left after healing. It was, however, remarkable how insignificant was the scar if the fungus shrivelled without ulcerating, thus proving that the granulation mass sprang from the superficial layers of the skin. These funguses were often attended by other forms of eruption, pimples and papules more or less scaly. Their first stage was a red pimple, which quickly formed a little scab, under which granulations sprang up. Sometimes there would be only two or three of the fungus growths, but in others as many as fifty.

Mr. Wallace says that some of his medical friends were familiar with the same eruption as occurring occasionally amongst the Irish peasantry, and he says that Mr. Carmichael had described it in his work on Syphilis as being non-venereal, but without being aware that it was curable by mercury.

As regards the influence of treatment, Mr. Wallace sums up his experience as follows: "In the whole catalogue of maladies which are capable of being cured or relieved by mercury, I do not know any which exhibits the value of this mineral more remarkably than the disease in question. I believe it matters not whether this valuable agent be employed internally or externally. . . . The disease immediately shrinks *as soon as the slightest mercurial action in the system is manifested.** I generally retain the patients under care five or six weeks, and use the remedy so as to cause gentle but marked influence on the system."

Now if it be suggested that Mr. Wallace was after all describing and treating nothing other than a framboesial form of syphilitic eruption, what would be the reply?

1. The patients denied having had any venereal disease. They were, however, most of them at the age and in the

* The italics are in the original.

class of life prone to suffer from syphilis, and there is no record of inspection of the genitals.

2. There were none of the other usual indications of syphilis, such as sore throat or mouth, or pains in the bones. It is certainly remarkable that so large a number of patients should have been free from these concomitants if the disease were syphilis. Nevertheless it is well known that in some patients this disease does affect the skin severely and yet exempts the mucous membranes, or *vice versâ*.

3. In but few of the cases was any contagion traced, and in some, since the patient slept with a bed-fellow and did not communicate it, there appeared reason to doubt whether the malady was contagious. This is not an argument of much weight, and in two instances it is definitely recorded that the eruption did appear to spread by contagion.

In conclusion it may, I think, be said that in all probability the disease was syphilis and nothing else. We know that syphilis is sometimes attended by precisely this form of eruption. The mode of cure supports this opinion, and there is no proof that adequate care was taken to exclude error. Some of the patients are stated to have relapsed after apparent cure, and to have been again treated in the same manner, but in none is it recorded that the patient remained under observation for any long period.

Under the head of "Diseases likely to be confounded with Venereal," Mr. Carmichael records the case of a young unmarried lady "whose condition and morals placed her altogether beyond the reach of suspicion." Yet "her symptoms precisely resembled those which are undoubtedly of venereal origin." She had a number of large tubercles scattered over her legs, arms, and thighs, attended with discoloration of the integuments." The back of the pharynx was ulcerated and covered with white tenacious matter, "evincing a similar correspondence between the affection of the skin and the throat to that which I had often witnessed in venereal cases." Mr. Carmichael adds, "I merely regulated her diet, ordered five grains of blue pill every night, and three drachms of sulphate of magnesia every morning,

under which plan she had perfectly recovered before the 12th of July following (one month), when I ceased to visit her."

Can any one doubt that this young lady really had syphilis, and that she was cured of it (for the time) by a treatment which, although given on a mistaken diagnosis, was quite adequate to that result. Mr. Carmichael allows himself a double illusion: first, that a young lady's social position can be held to shut out the possibility of syphilis; and next, that five grains of blue pill given every day for a month is not a treatment likely to cause the disappearance of secondary symptoms. Three to five grains of grey powder daily is now known to be efficient for the treatment of syphilis. Nor is it quite necessary to assume that the young lady's morals were less firm than Mr. Carmichael assumed them to be, for there is no indication whatever that any search was made for an erratically placed chancre. As to the cure, it is to be noted that the patient was left as soon as the symptoms subsided, and no effort is recorded to substantiate the diagnosis by noting the sequel of the case.

[I do not suppose that this case is the one alluded to above by Mr. Wallace as recorded by Mr. Carmichael. I have, however, failed to find the proper reference, and it is not supplied by Mr. Wallace. I quote the case as affording to some extent a parallel with Dr. Wallace's observations on "Morula."]

ILLUSTRATIONS OF THE LAWS OF HEREDITY IN ARTHRITIC MALADIES.

ALREADY I have in ARCHIVES repeatedly referred to the laws of inheritance as explaining the differences presented by arthritic maladies. That inheritance of tissue proclivities is the most important of the complex influences under which the various maladies constituting this group are developed is my firm creed. We cannot make too much allowance for the possibilities in respect to transmutation in transmission under which an inheritance of gout may assume the form of chronic rheumatism, or may predispose to the acute form or to any one of many different types. It is a fallacy to suppose that the offspring will inherit precisely the malady from which one or other of his or her parents suffered. There are very many intervening influences which may modify the result.

Rheumatic Sclerotitis in a Girl—No family history of Arthritis obtainable—Twenty years later her mother the subject of severe Rheumatic Gout.

In 1874, when she was eight years of age, I had under care Miss S——, the daughter of a surgeon, on account of an affection of one eye which I diagnosed as "rheumatic sclerotitis." The condition—a dusky elevation on the sclerotic at a little distance from the border of the cornea—was so characteristic that I had a portrait of it taken. Miss S—— had also eczema around her mouth affecting the lips, and of this also Mr. Burgess made me a portrait. The question was whether, in the almost entire absence of any family history of either rheumatism or gout, I had any right to

claim these affections as due to inherited arthritic tendencies. Miss S—— was well grown and florid.

Under treatment both the sclerotitis and the eczema were cured, the latter having persisted a long time.

I did not see anything more of Miss S—— for twenty years. In 1896 her mother (æt. 64) came to me on her own account. She reported that her daughter, now aged 31, was in good health and quite free from her former ailments. Mrs. S—— herself came on account of inflamed eyes. She had a form of phyctenular ophthalmia, with a small ulcer on one cornea near to its edge exactly of the type which, when met with in elderly persons, I have been accustomed to associate with gout. She was in good health, "excepting a weak heart," and had for long restricted herself to whisky and water. She was, however, accustomed to take fruit and sugar freely. On inquiry I found that she had suffered much from lumbago, and that she was the subject of chronic arthritis of both knees, with lips on the condyles of the femur, and some effusion. She said that she had a chalk stone on one finger, but it proved to be only one of Heberden's nodes. One of her sisters was, she said, quite crippled with chronic rheumatism, and went about on crutches. I could still obtain no history of typical gout in the family. In spite of its absence, however, I think that what has happened in senility to the mother and aunt of Miss S—— may be allowed to be confirmatory of the diagnosis which was formerly given. The family are unquestionably arthritic, and I have little doubt that if the full history of former generations could be obtained, that there has, in the past, been gout as well as rheumatism.

(Under treatment by colchicum, aconite, and alkalies, Mrs. S——'s eye was well in a fortnight. January 23, 1896.)

Illustration of the Inheritance of Chronic Rheumatism.

A working tenant farmer, living in a very healthy and sandy district, is crippled by rheumatism. He is permanently lame from disease in one hip-joint. I was interested in ascertaining how he had acquired it. I was informed by a neighbour, who had known him from boyhood, that

he had been born in the district and had always been very temperate in his habits. My informant thought that the rheumatism might be in part due to frequent exposure to wet and too often keeping wet clothes on, but, he said, "it is in his family." "Why, sir," added he, "I remember his grandfather, who was crippled with it far worse than John is. John, when he was only a boy, had to go with the team, taking his helpless grandfather in the waggon to show him what to do. The old man used to be put down in the corner of a field to watch the lad. For many years before he died he was so crippled that he could do nothing himself in the way of work." No doubt the frequent exposure to wet and cold of farm labourers has much to do with their tendency to chronic rheumatism. Still, as seen in the above narrative, it is probable that hereditary tendency is an important predisposing influence. It has been far too much the habit of authors on rheumatism to ignore inheritance. Some even go so far as to admit inheritance in the case of gout only.

Although in the disease of which the above is an example we very frequently fail to establish any history of true gout in a predecessor, yet from general experience I am inclined to believe that it has been present in most. In the yeoman population of England in former times gout was common, and many who are now no better than labourers are descended from those who were in a position to earn gout. I feel confident that in most cases of severe forms of local rheumatism, and also in the crippling form affecting many joints, there is a mixed inheritance, and that true gout has rarely been absent.

Inheritance of Gout and Rheumatic Gout—Patient a lady of 39—Rheumatic Fever ætat 20—Rheumatic Gout in the jaw-joint of one side with recurring attacks of very severe pain—Gastric Neuralgia with paroxysms resembling Angina—The patient's mother the subject of the same form of jaw-joint disease.

It may a little diversify the style of narrative if I allow the subject of this case, a very intelligent lady, to detail

her own case. I will append to her statements a few supplementary notes of my own.

"My father had for many years valvular disease of the heart, but died of cancer in the face at the age of eighty-nine. He had fourteen children, ten of whom are living. My mother, his second wife, was for many years a sufferer from rheumatism. She died at the age of seventy-one, the cause of her death being given as rheumatoid arthritis. My eldest brother suffers from gout in the feet, and several of my brothers and sisters are troubled more or less with rheumatism.

"I am now in my fortieth year, married, and have one child (æt. 9). In my girlhood I had several very severe illnesses, chiefly fevers. Had rheumatic fever when I was about twenty-two. About the same time I began to suffer with what seemed a form of indigestion, the attacks generally coming in the night, suddenly, causing a very weak and disturbed action of the heart with flatulence and other distressing symptoms. At times I had very severe attacks of pain in the region of the stomach, which my physician called 'neuralgia in the stomach.'

"About six years ago I began to have trouble in the left side of my face, just at the joint of the jaw. At first it was only pain and stiffness about the joint. After a time I began to have dreadful attacks of pain, lasting sometimes several days, the acute pain then subsiding, but the soreness and stiffness continuing. I am never entirely free from the pain, and there appears to be a little swelling of the joint.

"I was at first treated for 'neuralgia,' afterwards for 'rheumatic gout.' The trouble with my stomach and heart seems to be increasing; and that in my face, after five years' treatment by skilful physicians in America, is certainly no better."

The writer of the above was a stout lady, aged 39, whom I saw with her brother, who was a physician. I saw her only once, and the following are some additional facts which were elicited at the consultation. I am sorry that I have not preserved any definite record as to the state of the heart, but I know that I was assured that there was no valvular disease.

She has been dieted for her stoutness. There is very considerable thickening about the left jaw-joint. Her jaw was very stiff when the attacks were on, and is still so somewhat. Is liable to a distressing sensation at the stomach and heart, which occurs frequently in bed after the first sleep. It is, she says, a dreadful sensation, as if of impending death.

make an exception to my rule in favour of a case, the photographic portrait of which has recently been placed in the Clinical Museum, and the particulars of which have been recently recorded (since Mr. Stonham's paper) by Dr. Weldon Carter, of Preston.

Myositis ossificans affecting symmetrically the Pronator Radii Teres of each Forearm, and no other Muscle.

A little boy of 3, Master C. D. W—— was brought to me on June 6, 1893, on account of a deformity of his left forearm. The limb was quite shapely, and nothing could be observed at first sight further than that it was always kept in strong pronation. He could take hold of any object, but always did so with the back of the hand uppermost. On examination, the forearm was found to be fixed in this position. There was nearly an inch of shortening in the length of the ulna as compared with the other limb. The styloid process of the ulna projected somewhat. In the other arm supination appeared to be restricted, but was by no means lost. The elbow admitted of free flexion, but the biceps muscle on the left side was so small that at first I thought that it was absent. It was doubtful whether or not the upper third of the shaft of the radius was absent. There was certainly a want of support in grasping the forearm in this position, but two observers differed as to whether it was present or not. The head was certainly present. There was no history of any injury or inflammation. The condition had first attracted attention about six months before the boy was brought to me, but some degree of it might not improbably have been congenital.

In February, 1896 (nearly three years after the above notes), Mrs. W—— brought her boy to me again. She was apprehensive that the tendency to permanent pronation was increasing in the other arm.

On the second consultation I discovered a condition which had escaped notice at the first. Obliquely down the upper half of the front of the forearm there ran a bony ridge. This

ridge on being traced appeared to correspond exactly with the position of the pronator teres. At its insertion into the radius and through its whole course the muscle appeared to be converted into hard bone. At its upper origin this was less definite, but at this part also it was hardened. The ridge thus constituted projected and was easily felt. Evidently the muscle was much shortened.

In the other arm, which was in a position of semi-pronation, and could not by any force be supinated, the muscle could be felt to be hard and contracted, but was not certainly bone.

I now made a careful examination of the rest of the boy's muscular system, but could find no evidences of ossification elsewhere. He was slightly knock-kneed, but this was the only defect which could be found. He was in excellent health, and could use all his limbs vigorously.

The difference in length of the two ulnæ was not so great as in the former observation. It was a little difficult to measure accurately owing to the position of the forearm, but I could not make it more than half an inch. The styloid process of the ulna projected strongly on the back of the pronated wrist.

I do not know of any facts as to the associations of the pronator teres which may in any way explain a symmetrical affection of it whilst all others remain sound. There is great reason to fear that the right arm will follow the course of the other.

Although in Mr. Bilton Pollard's case the results obtained by excision of some of the bony plates was not very encouraging, yet seeing that in my patient only a single muscle is affected, there is much to be hoped for from such a procedure. The case the most nearly approaching to the one above detailed with which I am acquainted is one recorded by myself in 1860. The patient has subsequently been under observation at many hospitals, and recently I have been indebted to Mr. Willett for another opportunity of seeing him. In this case undoubtedly the changes commenced during intra-uterine life, and they have been restricted to the upper extremities. Although symmetrical,

The proved occurrence of true gout in any one near relative is sufficient to justify the assumption that the liability is present in the whole family. Whatever may be the maladies from which the relatives of such an individual may suffer, it is wise to suspect that a gouty inheritance may take its share in their causation. In the present instance we have what is very usual—a man suffering from true gout, and two women, his mother and his sister, from severe rheumatism. The difference in dietetic habits in the two sexes to some extent explains this. It is, however, by no means certain that some of the daughter's attacks in her jaw-joint had not been of the nature of true gout. The pain was described as having been very severe, and the attacks short and repeatedly recurrent. I was told that the patient's mother had suffered from stiffening of her jaw-joint on the same side and with precisely similar symptoms.

It is to be noted that although only one member of the family was known to have had true gout, many had suffered severely from chronic rheumatism, and that our patient had had rheumatic fever in youth.

SOME EXAMPLES OF MYOSITIS OSSIFICANS.

My motive for writing upon this topic at the present time is that I have now under observation a most interesting example of the ossification of a single muscle in each forearm. The patient has already been produced at one of my demonstrations, and may very probably attend again. I have never seen a quite parallel case. Mr. Stonham, who has made a much more nearly exhaustive analysis of the literature of the subject than I have been able to attempt, tells me that he has not found any such on record. The symmetry of the affection, and its limitation to a single muscle, seem to throw valuable light upon the laws of caustion, and to place the malady in that group in which there is an inborn tendency in certain structures to take on morbid changes. We may put aside all such suggestions as rheumatism, injury, and indeed for the most part all post-natal influences. We see the condition beginning in early life, slowly aggressive, and involving similar structures in parallel parts. The analogy is with retinitis pigmentosa, xerodermia pigmentosum, and some other affections.

After mentioning the facts as to the case referred to, I will take the opportunity of appending some particulars of other cases which have formerly come under my notice. No attempt will be made to recapitulate what others have written on it. As my readers are aware, my Archives are for the most part frankly egotistical, and concern only facts which I have myself examined, or opinions which I have myself formed. To attempt more would be foreign to my plan and my capabilities. In the present instance I am the more willing to avoid any endeavour to review the cases recorded by others because Mr. Stonham has done it exceedingly well (see the *Lancet*, vol. ii., 1892). I shall, however,

Still tracing the case backwards, I will conclude with what ought perhaps to have come first—the description of the patient's state when a boy.

The following is taken verbatim from the Hospital Reports of the *Medical Times and Gazette*, March 31, 1860:—

"*Case of Multiple Exostoses with Ossification of Fascia in various parts, and Ichthyosis.*

"J. H——, a boy of about twelve years of age, was admitted under the care of Mr. Hutchinson on account of the following very singular state of things:—He had previously been a patient at several other hospitals. In the left upper extremity the shoulder, wrist, and carpal joints were firmly anchylosed. The left humerus was shorter than its fellow, and running down its whole length on the outer side was an irregular ridge of bone, which projected about two inches below the end of the outer condyle. Over this ridge the skin was adherent to the bone, and was thickened, and in a condition much resembling certain forms of ichthyosis. The motions of the elbow-joint were much impeded. In the fascia of the forearm were large plates of bone two or three inches in length, and amongst the tendinous structures of the palm was a large nodular mass. Several small isolated deposits of bone were scattered about the fingers and beneath the skin of the back of the hand.

"In the right extremity the morbid conditions were not so extensive as in the left. There were nodular growths about the bones of the elbow, and large plates in the fascia above and below that joint. Some of them were easily movable from side to side, being, however, five or six inches long by two broad. One of them extended over the internal condyle downwards, and was attached to that bone, overlapping the inner aspect of the elbow, but permitting of motion.

"With regard to his history, it was stated that the midwife who delivered his mother observed the deformities immediately after the boy's birth, and that when three weeks old he was placed under care on account of them at the London Hospital."

In January, 1886, my valued friend, the late Mr. Sympson, of Lincoln, brought to me a boy who was the subject of this malady in its most typical form, and at Mr. Sympson's wish I exhibited the case before a meeting of the Clinical Society. The facts of the case were briefly published in the Society's Transactions, and subsequently with additional particulars by Mr. Sympson in the *British Medical Journal* for Novem-

ber 27, 1886. Further facts as to the progress were added in 1892 by Mr. E. Mansel Sympson. It happens, however, that although the case has thus in some respects been rendered very complete, the record having extended over six years, no detailed description of the state at the first examination has ever been published. It seems worth while, therefore, to now give the notes which I took at the time, and which were intended to have been inserted in the Clinical Society's Transactions, but were not sent in. They are the more valuable now as we possess information as to conditions subsequently attained. The boy is, I believe, still living. He was eleven years of age when the following description was written :—

Detailed description of Mr. Sympson's Patient—Generalised Myositis Ossificans, January, 1886.

On the left side of the neck, apparently in the upper part of the trapezius, almost close to the occiput, there are some irregular masses as hard as bone, which are quite movable when the muscle is relaxed. From them downwards there are spinous extensions and another hump more deeply placed. On the opposite side there are also some bony lumps, apparently not in the trapezius, but more deeply placed.

Passing down to the root of the neck in the dorsal region, there is on the right side a lump as big as a cherry, apparently attached to the base of the spinous process of the scapula.

The right shoulder seems to be quite fixed; there is no movement in any direction.

Extending from the humerus down the outer border of the axilla there is a nodular, bony mass visible through the skin, which passes from the humerus to the lower angle of the scapula. The scapula on this side has a limited range of motion.

On the back, on this (the right) side, near the spine, apparently fixed to the ribs, there are a number of low, broad-based, bony lumps, apparently exostoses.

There is an irregular bony crest which is visible through the skin, apparently developed in the posterior part of the iliac crest close to the spine, about two inches in length.

Passing again to the left side, we find limited motion at the shoulder joint, chiefly of flexion and extension; less involvement of rotation. It is remarkable that the pectorals are not affected on either side. The deltoids are wasted.

On the left side large bony masses are visible through the skin,

extending in an oblique line upwards and outwards, in the position of the lower edge of the latissimus dorsi. These masses in the lower part are quite fixed to the ribs. There is a slight outgrowth on the posterior part of the crest of the ilium, corresponding with that on the other side, but not nearly so large.

Although in both humeri the ossific growths are attached to the bone, there is no proof that they spring from it, with the exception of some which are found on the back of the bone, extending downwards from the insertion of the latissimus dorsi to the middle of the bone. These are much larger on the right side than on the left, and project as spinous outgrowths, overhanging their bases. It is possible that these may be in connection with the origin of the triceps. On the left side it is not certain that there are any lower than the latissimus dorsi.

In the lumbar region on each side, extending from the spinous process an inch and a half each way, there is a firm, bony plate, which appears to be immediately beneath the skin. This plate entirely prevents him bending the lumbar spine. On each side it presents a hard, rounded edge. It is impossible to say whether it is in the latissimus dorsi only, or whether it involves the deeper muscles.

The movements of the head are restricted, but by no means abolished. He can rotate, nod, &c., with moderate freedom. The sterno-mastoids are not affected.

There was a history in this case that the disease had followed a fall, and that the first observed symptom was a painful swelling over one scapula. This, however, was only the observation of his parents, and as the date given was but two years before my notes were taken, it may be suspected that the changes had been in progress long before the accident. This view is supported by the fact that the lad was the subject of a peculiar malformation of the great toe, which has been noted in other cases, and which was no doubt congenital, and which was present also in the boy's father. A peculiar feature specially dwelt upon by Mr. Sympson was that the ossification was often preceded by the formation of elastic swellings, which underwent shrinking as they became hard. Small nodules the size of buckshot were also observed. Subsequently to my description the boy broke both bones of one forearm. Repair was good, and without any unusual development of callus. Several years later he fell and bruised one patella, with the result that a hard swelling appeared on its surface.

This case is in all respects a typical example of the generalised form of aggressive myositis ossificans, and it is to be feared that it will progress to the conditions shown in the two celebrated skeletons which are preserved in the Museums of the College of Surgeons, London, and that of Trinity College, Dublin. The next case which I have to mention is like it in almost all details, and so also is the one described by Mr. Bilton Pollard in the *Lancet* for December 31, 1892.

(*To be concluded.*)

BACK TO THE TAXIS.

ALTHOUGH * I have ventured to plead for a return to the legitimate use of the taxis in strangulated hernia, I have always been a strenuous advocate for early operations. My plan has invariably been to give an anæsthetic in order to facilitate taxis, and, if success were not obtained, to proceed to the operation at once. Having said this clearly and repeatedly, I may confess that I did not suspect that what I have written on this subject was open to such misapprehension as is revealed in an article by Mr. Croly in the recent volume of the Transactions of the Royal Academy of Medicine in Ireland. Since, however, I have not made my meaning clear to Mr. Croly it is possible that I may not have done so to others, and I am accordingly thankful to be informed that it is needful to offer further explanation. The subject is one of the most important in the whole range of practical surgery. I should be grieved indeed if anything which I had said or written could be understood as sanctioning any delay whatever in the reduction of a strangulated hernia. Such reduction must be accomplished promptly, and if it cannot be done by the taxis it must be effected by aid of the knife. Let there be no doubt or dispute about that.

My object in the papers which I have recently written has been to protest, not against early operations—Heaven forbid! —but against the almost entire disuse of the taxis. I have shown by appeal to statistical facts that the recoveries after operations are not more abundant in present times than they were in the days of Mr. Luke, when a much larger number were saved by taxis. I have insisted that all statistical statements as to the treatment of strangulated hernia should give

* See papers in ARCHIVES, Vol. V., pp. 127, 133 and 378.

—first the total number, next the number reduced by taxis, with the mortality, if any, and lastly the number of operations with their mortality. It is useless to give the operation cases alone, since it is obvious that a surgeon who operates on all, without attempting the taxis, deals with much more hopeful material than one who reduces a third or a half of his best cases by its means.

I am sorry that Mr. Croly has not quoted from my own papers, but from some abridgment. Had he gone to the original he would have avoided some misapprehension. He censures me for having said that I "was sometimes amused to hear forcible taxis denounced," and says that he has "never seen anything to amuse in strangulated hernia," but, on the contrary, had always been "deeply impressed with the gravity of the subject." My expression was that I had been amused at the way in which candidates under examination dealt with questions about the taxis. I have, however, no objection to say now that I have often been amused to see it enjoined not to use force in attempts at taxis, since all the force which the hands can employ seems to me essential to success. As well recommend a man to drive a wedge without hitting hard.*

Mr. Croly says that Hey, Desault, Cooper, and Lawrence, were all in favour of early herniotomy, and expresses his

* I should be sorry to further hurt Mr. Croly's feelings—who, having "abolished the taxis" in 1869, now seems to regard everything pertaining to hernia as matter of solemnity, if not of gloom,—but what I have to relate has at any rate the justification of being a fact. It may, moreover, serve to show the light in which the past generation of surgeons regarded the taxis. I was assisting one of my teachers, the lecturer on anatomy at the York School of Medicine, by giving chloroform whilst he tried the taxis. After a long endeavour he felt the then well-known indication of approaching success, and, looking up with cheerful face, exclaimed—

"Not the bee upon the blossom,
(*I've forgotten the next line.*)
Not the poet in the moment
Fancy lightens in his e'e,
Kens the pleasure, feels the rapture,
That that gurgle gi'es to me."

After the gurgle came the reduction, and after the reduction the uninterrupted recovery of the patient.

[illegible] that I should differ from them. I do not differ [illegible] at the least, for they all used, advocated, and [illegible] the manipulations necessary for the taxis, [illegible] would do. It is from Mr. Croly that I differ, who [illegible] apparently with continued approval, an expression [illegible] himself in 1869, "that the taxis should be [illegible]." None of the authorities he cites ever dreamt of [illegible] terms. I grant that it is not fair to accuse Mr. [illegible] of a wish, in 1896, to entirely disuse the taxis, for in [illegible] part of his paper he speaks of it, in relation to the [illegible] in terms which I might myself have used. My [illegible] were, however, written in the belief that the teaching [illegible] present in vogue, at many of our schools, goes very [illegible] the length of abolishing the taxis.*

Whilst, however, I justify force, it is surely unnecessary [illegible] that I reprobate violence, and all approach to roughness, as much as Mr. Croly can do. The victory is to skill more than to force, although the latter must be used. On this account I regret exceedingly that instead of being carefully taught how to do it, the present race of students is but too often enjoined to avoid the taxis altogether, or to regard it with such fear that ill success in its employment is certain.

As regards injuries done by the taxis, the statistics quoted from Mr. Luke show that he obtained better results from the operation than are usual now, although most of the cases on which he operated had been submitted to patient endeavours at taxis, and a large number of the most hopeful cases had been eliminated and saved by its aid. Mr. Croly publishes a letter from Mr. Bryant, who writes respecting a former Guy's surgeon who was zealous and very successful with the taxis, that he (Mr. Bryant) found on consulting the P.M. records "those that had died from rupture of the bowel were all registered as having been under his care, consequently what successes he might have gained were more than neutralised by these fatal failures." Every one who knows much of the past of Guy's Hospital will know to whom Mr. Bryant here alludes; one of the best surgeons

* See a Conversation, Vol. V., p. 378.

who ever lived. The late Mr. Hilton, in common with Mr. Wormald at St. Bartholomew's, used to boast that he very rarely was reduced to operate on an inguinal hernia. That a man who uses the taxis freely is more likely than one who avoids it to have an occasional case of rupture of the bowel may be admitted, but that such accidents could be so frequent as to more than neutralise his successes may in the absence of proof be held doubtful. Rupture of the intestine by efforts at the taxis, if it occurs at all, will probably take place, not in the abdomen, but in the sac itself, and the operator would probably find it impossible, after it had occurred, to get the bowel back. My experience does not include a single example of it. I have already recorded the fact that I have never known a patient die after successful taxis for inguinal hernia, and only three after taxis for femoral.* Mr. Luke and Mr. James, both men of very large experience, have stated that they never knew a patient die after successful taxis. In the face of such statements it is impossible to believe that deaths after successful taxis are under any circumstances frequent.† In my student days we did not know that they were possible, but regarded a success by taxis as wholly without drawbacks, and as half equivalent to saving a patient's life. Yet our operation cases were not more fatal then than now.‡

Whilst I thus insist that the taxis has *per se* very little

* In one of these the patient was moribund when I saw her, and I was merely examining the tumour without any serious intention of reducing it when it slipped back. It was a neglected case in which the hernia had never been diagnosed and no taxis had been used.

† When Mr. Bryant writes, "In neglected cases of femoral hernia the taxis may be omitted in favour of early operation, for with early operation success would probably almost always be secured," I confess he is more sanguine than I am. "Neglected cases of femoral hernia" are most certainly unsuitable for the taxis, but I fear they also supply a considerable mortality even after the operation.

‡ I well recollect one of the first cases of strangulated hernia which came under my care after I had been elected assistant surgeon to the London Hospital. It was a large inguinal in a young boy. Having done my best at the taxis and failed, I requested my senior colleague, Mr. Nathaniel Ward, to be good enough to be present at the operation. Mr. Ward came at once, and after handling the tumour remarked quietly, "If you go on I think you'll get it back"; I did so, and got it back, and the child was forthwith well.

risk attending it—the real danger being in delay—I am concerned to reiterate my conviction that the *per se* dangers of operations are real and not inconsiderable. Statistics seem to fully prove what is in this matter *à priori* probable. The operation is not unfrequently, and especially in inexperienced hands, a difficult one. Unexpected conditions may be encountered which may cause prolonged exposure of the parts. Now and then there may be troublesome hæmorrhage. Above all, however, it is by no means proved that the exposure to the air of the peritoneum in a congested and more or less damaged state, can be done with the same impunity as when it was uninflamed. No one will rejoice more than myself when statistics show that the chance of recovery after operation is, in similar cases, equal to that after taxis, but for the present it is my duty to protest that the facts look much otherwise.

ON TOPHI WHICH ARE NOT CHALKSTONES.

It is to be admitted that to speak of a tophus * which is not tophaceous involves some defiance of etymology. The word has, however, obtained a conventional meaning as applicable to deposits in the tissues in connection with gout, many of which are only in part calcareous. It may, therefore, perhaps be permitted to forget the derivation of the word, and, to use it in the wider sense, as applicable to all gouty nodules and deposits, whatever their structure.

The object of the present paper is to ask attention to the frequency with which nodules and small local deposits and growths occur in gouty patients, or their relatives, which are not really chalky. Dr. Barlow's observations have made us familiar with what are known as "rheumatic nodules," which are recognised chiefly in children having arthritic inheritance. Between these and the true chalkstone of the gouty adult there appear to be many connecting links. The connection between gout and urate of soda has, it may be reasonably suspected, been made far too much of. Many changes are essentially connected with gout proclivities, in which there is no tendency to the deposit of urates in the tissues. True chalkstones are indeed a rare occurrence in gout, and are met with only in special forms of it. I do not wish to underrate their importance when they do occur and fully admit that they take rank as the crowning proof of gout. There is, however, I am sure, a great deal of gout which never attains to chalkstones. Such forms occur abundantly in those who inherit gout liabilities, but whose mode of life has contributed nothing to increase them.

* Ogilvie's Dictionary defines a tophus "as a *soft* tumour on a bone, or a concretion in a tissue," and does not insist upon calcareous qualities. As a matter of etymology the terms "calcareous," and "chalkstones," are neither of them correctly applied to concretions of urates.

They are, however, by no means confined to such, but are not infrequently met with in adults in whom inheritance has been assisted by life-habits, but in whom the malady has not reached any high degree of intensity.

In order to avoid prolixity, I will attempt to enumerate in groups the lesions to which I more particularly refer.

I. *Non-calcareous nodules in the ears, in the subjects of declared gout.*—It is very common to see in the ears of those who have had attacks of acute gout, and in whom it may be chalkstones are at the same time present, little nodules which look as if they ought to contain chalk, but which never do and may disappear entirely after a time. These are yet more common in those who are threatened with gout, but have never had definite attacks.

II. *Fibroid thickenings, and little lumps on the hands of those who suffer from gout.*—These are very common and may be arranged symmetrically. They vary with the health as regards gout, and are at times painful. They may last a long time and then disappear, or may be permanent. They often occur close to joints, but not always. They seldom or never become calcareous.

III. *Fibroid thickening of Bursæ.*—These are not unfrequently met with in various forms of gout, and often quite independently of local irritation. They sometimes inflame and may suppurate, but a tendency to thickening rather than to fluid effusion is their chief feature.

IV. *Gelatinous deposits sometimes diffused and sometimes nodular.*—These are chiefly distinguished from the fibroid ones by their softness, and they occur under much the same conditions. The best example of them is that of the girl Mabey, whose case is fully published in my Clinical Illustrations. She inherited gout. I cut out some of the gelatinous deposits near her finger-joints.

V. *The rheumatic nodules of Barlow.*—Often met with on periosteum, fasciæ, and beneath the skin in children who inherit arthritic tendencies.

VI. *The nodules met with in sclerodermia.*—These are clearly similar to the "rheumatic nodules," and are possibly identical with them.

VII. *The lumps which often accompany Dupuytren's contraction of palmar fascia.*--Not unfrequently there is fibroid hypertrophy as well as contraction, and it seems highly probable that there is a close association with the inheritance of gout.

VIII. *Lumps developed in tendons.*—These occur most frequently in the tendo achillis, but may be met with occasionally in other tendons. It is sometimes difficult to tell whether they are of syphilitic or gouty origin.

IX. *The indurations in the skin of the hands which constitute "the Judson Bury group."* (See ARCHIVES, Vol. V., p. 237, and Museum Portraits.)—These occur with inherited gout.

X. *The livid indurations in the skin which have been described as sarcoma melanodes.* (See ARCHIVES, Vol. VI., p. 11, and Museum Portraits.)—These occur in adults with inherited and acquired gout.

It is to be understood that these various conditions are often met with in the same case. Sometimes they are in conjunction with indisputable indications of true gout and sometimes with the history of inheritance of gout or rheumatism. They appear to form important links of evidence as to the mutual relationships of these two chief types of arthritic maladies.

Having thus sketched out the subject, I will proceed to the narration of cases in illustration.

CASE I.—*Mrs.* C—— (*portrait of ear and hand*)—*Paternal inheritance of Gout—Patient a woman of 43—Last-joint Arthritis—Bursal enlargements—Tophi gelatinous and fibroid—Much crippling—Two aunts also crippled.*

I saw Mrs. C——, æt. 43, on March 13, 1877, when a sketch was taken of ear and hand. It was a case of cold extremities and "last-joint arthritis," with remarkable tendency to the formation of little semi-solid gelatinous swellings over the diseased joints. These were present over each olecranon and over many of the finger joints. The last phalanges had been displaced forwards, and she said that the pain was

much less since they had been so. The olecranon bursæ had inflamed and suppurated. She had, when I saw her, an inflamed nodule in one ear, and said that she had had others on both sides. She had recently often had attacks of congestion of the eyes; the lids would swell at night so that she could scarcely see. She was very dyspeptic and flatulent. One of her brothers was a pewterer, and accustomed to drink freely, but he had never had gout. Her father, however, had suffered often from gout in his feet.

The condition of Mrs. C——'s hands was very peculiar. All her fingers were stiff and crippled, and they were all slanted to the ulnar side. The nodules were everywhere arranged just like common tophi about the knuckles and phalangeal joints. They were present also on the ears and the elbows. None of them appeared to be really earthy, and most were quite soft, as if consisting of gelatinous material. Some of them resembled small bursæ.

Mrs. C—— told me that two of her aunts had had their hands crippled like her own. She had herself been liable to suffer from attacks which she called rheumatic, during which all her joints would become stiff, but of these she had been, during the last four years, comparatively free. In girlhood she had been stout, but was now very thin. On one occasion she had had severe hæmorrhage from the throat (possibly hæmatemesis), and both she and her father had suffered from piles. Her case much reminded me of the Tretton family, in which two brothers and a sister, with two of their aunts, were crippled in a very similar manner.

CASE II.—*Thickening of the cellular tissue over the finger joints in connection with gouty inheritance.*

Mr. H. R. E——, aged 28, a healthy married man, of dark complexion, who was under my care for psoriasis, presented a condition of thickening in the subcutaneous cellular tissue over the knuckles, or rather the backs of the joints between the first and second phalanges. The condition was quite symmetrical, had existed for years, and caused no inconvenience. There was not any proved mischief in the joints themselves, and the fingers could be freely moved. Nor

was the skin itself, or at least its superficial layers, in the least diseased. The pads of thickening could be easily moved about. On a few fingers there were other pads much less circumscribed, and placed not at the back of the joints, but on one side. Mr. E——'s father and grandfather had suffered from gout, the former most severely and repeatedly from true gout, for twenty-five years. Mr. E—— himself, however, had never had any form of gout or rheumatism, nor had his brothers, so far as he was aware.

Cases III., IV., and V. will be found recorded in ARCHIVES, Vol. V., p. 235, and I need not here repeat their details. They are all illustrations of Group X.

Cases VI., VII., VIII., and IX. are examples of Group IX., and will be found in ARCHIVES, Vol. V., pp. 237–240. Of these, Case VIII. (Mr. R——, Case VI., p. 239) is a most instructive one, since the history of gout was strong and the conditions very peculiar. I am in a position, thanks to Dr. White of Nottingham, to now add some further particulars. Not long after I had seen the patient he had an attack of jaundice, and after it the indurations in the skin melted away, leaving the latter quite soft and healthy.

In the case since recorded by Dr. Crocker under the name "Erythema induratum diatinus," and which was a well-characterised example of this malady, the indurations have also entirely disappeared. Dr. Crocker has been kind enough to show me his patient.

CASE IX.—*Nodosities in cellular tissue, symetrically placed on shins, in palms, &c.—History of previous Syphilis.*

Mr. John F——, 41, single. It is two years and a half since he had a chancre, followed by an eruption and severe iritis. He was treated by a homœopath. The iritis ended in closed pupils, and Mr. Bader did an iridectomy in right, and attempted one in left. He got well in about a year, but continued some treatment till ten months ago. He knows of no gout in the family. His mother is rheumatic, but not severely so. He himself has had some stiffening in joints at times, but never much. He had ex-

cellent health before the syphilis. For eight or nine months he has had developing in certain situations, to be mentioned, curious nodosities in cellular tissue and in periosteum. These are accurately symmetrical, and have throughout been painless, or almost so. They began on his shins and in the palms at about the same time. They now affect both palms, both elbows, the lines of both ulnæ, and the surfaces of both tibiæ. In the palms they look like the beginning of contraction of facia, but on examining them there is no contraction, but a group of roundish, very hard nodosities which are quite movable. Along the line of the ulnæ, on both sides are similar nodosities, some loose, some adherent to the bone. Over the olecranon, in the position of the bursa, on the conch is a large knotted lump, quite movable, and over the inner condyle another. Along the spines of the tibiæ are numerous little indurations, some almost spinous, some exactly as if the bone had been broken and set with a little irregularity. Over some of these which are unquestionably bony there are other cellular indurations like little solid bursæ.

He thinks that these lumps are all getting smaller and softer (without treatment). He looks well, and has no other indications either of gout or of syphilis.

Dr. Barlow was kind enough to see this patient, and the following is an extract from his letter :—

"The characters of these swellings (their symmetry, painlessness, mobility, spontaneous subsidence) are quite those of the rheumatic nodules. As to the bony thickenings on the front of the tibia, I can give no opinion.

"As to the anatomical site of rheumatism nodules, I have satisfied myself by several examinations that in positions where it might be thought that they had begun in the periosteum, they have really begun in the fibrous tissue above the periosteum, viz., in the aponeurosis. This question could only arise, of course, with respect to the fixed ones, which are firmly bound down to the bone. Perhaps the periosteum might become secondarily affected."

ON ANCHYLOSIS WITH BUT LITTLE DISABILITY.

I HAVE at different times recorded a good many examples of stiffening at the shoulder-joint, partly to illustrate its frequency and the peculiar conditions under which it occurs, and partly to show how very little is the inconvenience it entails. An instance of it which has just come under my notice is of remarkable value in both these directions. A patient who had to strip to show a syphilitic eruption, exhibited slight awkwardness in taking off his coat. I noticed that he twisted his body a little in accomplishing certain movements. When his shoulders were bare I saw that, on the right side, the deltoid and scapular muscles were atrophied, the head of the humerus being covered only by skin. It proved to be firmly anchylosed to the scapula. I asked him what he could do with the limb. "Anything," he replied, "it is as good as the other," and he proceeded to execute a variety of movements with great ease, making his scapula move with extraordinary freedom. "You cannot put your hand behind you, or into your trousers pocket." "Yes I can," and he at once did both, though with some little difficulty and with the limitation that he could not get it higher on his back than just to button his braces. "I would rather have the other stiff too, than have what I've got now." This unusual facility in the use of an anchylosed shoulder had resulted from the fact that the stiffness had been produced in early life. I was told that it had followed an accident in which he was pushed down and the shoulder trodden upon. There had never been any abscess, and he utterly denied having ever had any pain. There was no proof of detachment of the epiphysis, and the humerus was

not shortened. The head of the bone did, however, seem to have lost its roundness.

The explanation of the readiness with which the shoulder stiffens is, not improbably, that the patient never recognises that he is not using it. He soon acquires the habit of moving the scapula, and thus leaves the joint itself entirely at rest. If the slightest pain is caused by moving the joint he unconsciously avoids doing so, and thus at once escapes pain and permits stiffening.

I have known two instances of anchylosis of the elbow-joint in left-hand persons in whom the disability entailed was not sufficient to induce the patient to use the right arm.

One of these was in an artist, who in boyhood had scrofulous disorganisation of the left elbow. After prolonged treatment the elbow stiffened and the sinuses healed. When a grown-up man he always held his brush in his left hand, and, in fact, did everything requiring delicacy of movement, although his right limb was quite sound.

In the second case I had myself treated a girl of fourteen for suppuration in the left elbow. After keeping the limb in plaster of Paris for a long time and sending her to the seaside, anchylosis resulted. I saw her as a married woman, aged 20, six years later. The joint was quite fixed and the soft parts sound. She assured me that this was the more useful limb, and that she always employed it in preference to the right one.

Such facts show how deeply implanted in the nervous system is the preference of one hand to the other. It must be remembered that in both of these cases the left limb had been wholly disabled during a long period of treatment. It might have been expected that during this period the right extremity would have become educated; but it was not so, and as soon as the left limb was liberated from splints it was again used preferentially.

These cases may also be allowed to emphasise the fact that very useful limbs are obtained when anchylosis of the elbow is allowed to take place. I have never seen a case in which, after an excision, the patient preferred the arm which had been operated upon. They also show that the tendency

to strumous affections beginning in childhood is often temporary. The woman was in good health when I last saw her, six years after the disease. I do not know anything of her present state of health. Respecting the artist, I may add to what I have already stated that he is now a man of sixty-four, and that he has never shown any liability to struma since his elbow got well.

ON THE PASSING OF GALL-STONES.

THE wood-cut illustration which is appended exhibits the exact size of a gall-stone which safely passed through the intestinal canal. It has been given me by Mr. R. G. Hogarth, of the Nottingham General Hospital, to whom I am indebted for the particulars of the case. The patient, a man of middle age, brought the stone to Mr. Hogarth with the statement that he had passed it after a fortnight's trouble. There was no history of any gall-bladder or liver symptoms, nor had anything occurred to indicate the stage during which the stone was making its way from the bladder into tlie intestine. The patient had experienced three days of obstipation, after which, under the influence of aperients, the bowels had begun to act again. It appeared probable that the stone had been lodged in the lower bowel for ten days, and whilst there it caused much annoyance and pain. When at last it escaped, the man regained his usual good health. At no time had the symptoms been severe enough to induce the sufferer to seek either medical or surgical advice, and he had thus escaped even the proposal of a laparotomy.

As I have many times pointed out, and as is now generally admitted, the stage of escape of a gall-stone into the intestines is very often unattended by symptoms. Although the present is a very definite example of their absence, it is by no means very exceptional. The case is, however, considering the size of the stone, a very remarkable instance of slight symptoms throughout. Although the case is, with many similar ones, proof as to what is possible by natural means, we should of course be very foolish to allow it to seem conclusive against the necessity for operation in any case. The

calibre of the intestine may vary much in different individuals, and with it the prospect of natural relief. Of far more importance, however, is probably the difference in different persons as to tendency to spasm. In women probably, as a rule, this tendency is greater than in men, and hence the greater severity of symptoms in them. Recognising,

however, that it is the spasmodic closure of the gut on the stone which causes the pain and prevents the onward progress, we have, I think, a clear indication for treatment. The patient should be kept for long periods under the influence of an anæsthetic, and the bowel below should be distended by injections of air or fluid. Distention from below is clearly what is wanted.

In publishing this case I have also the pleasure of being able to mention another which has occurred since my last reference to this topic, in which an operation was successfully performed. It was in the practice of my son at the London Hospital. The patient was a woman; the symptoms were severe, and the stone was found very firmly gripped in the small intestine. The recovery, although complete, ultimately, was, I am told, for some time doubtful, as the patient was very low.

So far as I know, the deaths and the recoveries at the London Hospital, after operations of this class, have been in equal proportions, and this not improbably represents the experience of surgeons in general.

It is much to be desired that those who have charge of the post-mortem rooms at our large hospitals would for a few years bring before the Pathological Society, as card speci-

mens, every example of a gall-stone found plugging the intestine which may come under their notice. By this means we should soon be put in possession of trustworthy facts by which to estimate the amount of danger which attaches to these cases when left to the physician.

DISEASES OF ARTERIES, VEINS AND LYMPHATIC TRUNKS.

Symmetrical Thrombotic Phlebitis of the short Saphæna Veins during the course of secondary Syphilis—Contusion suggested as an exciting cause—Gout.

A gentleman who was under the treatment of a colleague for secondary syphilis, consulted me because, as he said, he had had "inflammation of the veins of his legs." He said that he had had severe pain in one of the legs and more or less in the other, and had been for a time quite unable to walk. He naturally attributed it to his syphilis. Now nothing is less common than phlebitis in the course of syphilis, and I became much interested in his case. When he had undressed, I found sure enough that he had had thrombotic phlebitis of the short saphæna in both legs. In neither was the affected vein in the least tortuous, nor had he varicose veins anywhere. At the back of each leg, in the middle line, the inflamed and plugged saphæna ran up the middle, from a little above the heel to a little below the popliteal side, and on both it was as big and almost as firm as a cedar pencil. The stage of tenderness had pretty much passed by, and there was no redness of the overlying skin. I was assured, however, that at an earlier period there had been much tenderness and some redness.

I now pressed my patient as to whether he had no explanation to give of this most unusual condition—a symmetrical inflammatory thrombosis of veins not previously diseased. The following most important bit of evidence was given. One day Mr. —— had been galloping a young and restive horse in a field, and he was at the time wearing new gaiters, which nipped him at their upper borders. On dismounting

he found that he had a tender spot just where his gaiters had pressed on the back of the upper part of each calf. Next day the tenderness had increased, and during the next week it spread downwards and produced a line of tenderness down the whole calf. He was now told that his veins were inflamed. In one leg the pain was most severe and of an aching character, but on the other side it was but little. For some time he had been quite unable to walk, and had been kept awake at nights. It was three weeks after the beginning of the phlebitis when I saw him.

There seems no reason to doubt that we have here a correct assignment of the exciting cause. The affected veins had been compressed and irritated at their upper parts, and their thrombosis had progressed subsequently from above downwards. The pain, tenderness, and redness of skin had sufficiently proved that the condition was caused by inflammation. The recognition of the predisposing causes was, however, more difficult. The man was the subject of syphilis, and he was taking mercury. It is possible that one or other of these factors might have predisposed. On the other hand, the man was of gouty family, and had himself experienced some attacks. Gout is a well-recognised predisponent to thrombosis of veins.

Pseudo-hypertrophy following Obstructive Phlebitis.

A gentleman, named G——, who had suffered from typhoid fever in 1889, had had obstructive phlebitis of the left femoral vein after it. When I saw him in 1895 the whole of the left limb was decidedly larger than the right, but it was at the same time, as he said, definitely the weaker one. The superficial epigastric veins were enormously enlarged.

Hypertrophy of one limb, probably secondary to Venous Obstruction.

Mr. A. W. C——, a stout, muscular man, aged 28, was sent to me by Dr. Robinson, of Sheffield, in March, 1889. Six years previously the right leg was found by his tailor to be

larger than the other. His surgeon measured the limbs, and said he could not tell whether the left was too small or the right too large. Six months before his visit to me, he was laid up with a swelling of the thigh, which kept him to his bed for a fortnight and in the house for six weeks. The thigh had been aching all the summer, and then rather suddenly swelled up, until it measured twenty-six inches in circumference. His doctors thought there was an abscess in the outer part of the thigh, and prepared to open it; but the swelling subsided. Since then he had not walked much, because exercise made his thigh ache. There was no history of syphilis.

Lymphangitis first of one lower limb and then of the other —Difficulty in diagnosis from phlebitis.

The following notes refer to a case in which a middle-aged man had great general œdema, first of one lower extremity and then of the other. The case of Mr. M——, to which I refer, was published many years ago.

Mr. J. D——, whom I last saw on December 14, 1894, called on me again on April 15, 1895. After three months' confinement in bed, he had been up and about for nearly a month. The œdema of his legs had, under the use of the Martin's bandage, almost wholly disappeared. He was in good health, and had been so throughout. His legs, especially the one last attacked, were still liable to swell if he stood long. No enlarged veins had made their appearance, nor was there any disease of the lymphatic glands. Mr. D——'s case is a close parallel to that of Mr. M——, the points in both being that œdema of one leg began in connection with a slight injury, and, travelling up to the hip, involved the whole extremity from haunch to toes in a condition of firm, semi-solid œdema. In each instance, just as the œdema was subsiding in the leg first attacked, the affection crossed over and involved the other extremity from above downwards. In Mr. M——'s case, enlarged lymphatic trunks could be felt beneath the skin of the upper parts of the thighs and lower part of abdomen. These were of such considerable size that one surgeon thought they were

he found that he had a tender spot just where his gaiters h. pressed on the back of the upper part of each calf. Next d. the tenderness had increased, and during the next week spread downwards and produced a line of tenderness do the whole calf. He was now told that his veins were flamed. In one leg the pain was most severe and of aching character, but on the other side it was but little. F some time he had been quite unable to walk, and had b kept awake at nights. It was three weeks after the beginni of the phlebitis when I saw him.

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Hypertrophy of one li ary

Mr. A. W. C——, a sent to me by Dr. Rol Six years previously the

plugged veins. I felt confident, however, that they were not so, for they were all exactly of the same size, as hard as whipcord, and so abundant as to constitute almost a network. In Mr. D——'s case, as he lived at Hemel Hempstead and I did not see him at home, I had no opportunity of ascertaining whether anything of this kind was present. I could not detect anything on November 14th, the date of my seeing him last, when the limb first affected was getting better. At this date I warned him that the other leg might be affected, and a fortnight later he wrote to me to say that my prophecy was fulfilled, and that he was again laid up in bed. He was, as has been said, confined to his bedroom for nearly three months. In the case of Mr. M——, I have known the patient for twenty years since the attack, and he has not suffered from any relapse. I have no doubt that these cases are due to lymphangitis, with the temporary occlusion of the main trunks, and that they are allied to erysipelas. In neither case, however, was there any redness of the skin or any material constitutional disturbance. Both the patients were Jews, and both probably gouty. In neither of them, as has been stated, was there any dilatation of veins after recovery, and in neither could I ever detect any plugging of veins.

In a third case, which I have recently been attending in consultation with Dr. ——, of Hendon, a gentleman who, from build, age, etc., might have been the brother of either of the preceding, and who, like them, was a Jew, has had an attack of œdema of one leg in connection with well-recognisable thrombotic plugging of the long saphena, and with affection of the other limb in a precisely similar manner just when the first was recovering. In the limb attacked second the saphena vein could be felt as a cord from the groin almost to the ankle. This case differed, however, from the other two in the small amount of œdema which was present. The patient, like the others, was a gouty man, and he had been for some years the subject of prostatic disease necessitating the use of the catheter.

NOTES ON MALADIES WHICH ARE LIABLE TO RECUR PERIODICALLY.

In the endeavour to find data for a natural classification of morbid phenomena the question of liability or otherwise to recurrence claims prominent attention. The affections, which disappear almost spontaneously but come again repeatedly, without, it may be, any very obvious cause, although they affect very different structures, will probably be found to have a real relationship, and should be grouped accordingly. Here, as in many other investigations, diseases of the skin afford us the best opportunities for observation. The dermatologist is familiar with the facts in reference to recurring herpes and what he calls erythema multiforme. He knows also that lichen planus may recur after years of perfect health, and that psoriasis, although it may never get wholly well, is liable to definite exacerbations which amount to recurrences. From the study of these and of many others which come frequently under his notice, he is enabled to offer suggestions to the general physician as to the true nature of other and less obvious phenomena. Nor must he in turn neglect facts furnished by other departments of practice; for in seeking to explain these recurring and transitory affections of the skin he will do well to take cognisance of all other maladies which seem to obey parallel laws. The recurrent iritis, which is an appanage of rheumatic gout, is in some of its features very analogous to them. It not unfrequently recurs year after year in the same month, or it may, like the recurrent erythemata, have much longer or much shorter intervals Other forms of inflammation and congestion of the eye, in association with rheumatism and gout, are also recurrent. The paroxysms of gout itself are in many persons definitely

recurrent and in connection with the seasons, and the same is to be asserted regarding the much less frequent cases in which patients are liable to repeated attacks of rheumatic fever. Epilepsy is recurrent, and in many persons its explosions appear to wait until a certain more or less definite period has elapsed. The patient having had an attack is confident of immunity for a time. The same may be asserted as regards bilious attacks, sick headaches, and hemicrania, although these, it may be admitted, appear to have a closer relation with errors in diet and other accidental influences than many of the affections which have been named. The explosion of a severe catarrh is, however, after all, the closest parallel which I can suggest.*

We may classify recurring eruptions in the skin in relation to the type assumed and to the parts affected, whilst we assert that there is a link which connects them all, as being probably of neurotic origin. The subdivisions are of comparatively little importance as against the distinct recognition of relationship. In one case the eruption may be a bullous one (sago-grain or cheiro-pompholyx), in another it may be the well-margined patches of erythema multiforme with or without vesications, in another it may resemble erythema nodosum, whilst in yet others it may assume an almost purpuric type. Individual peculiarities may perhaps explain some of these differences, whilst in others varieties in the complex influences of causation may perhaps be present, and capable of recognition by patient study. Thus it may be that the purpuric cases have a closer connection with inherited gout than the others.

Nor must we neglect to study the means of cure and of prevention as being likely to afford us some clue

* A not uninstructive comparison may, I feel sure, be drawn between the liability to sick headaches and to catarrh. In each instance the phenomena evoked are those of a nerve-explosion. In the one instance there is greatly increased secretions from some surface and evidences of vascular dilation; in the other, a state of vascular tension is accompanied by entire arrest of secretion or perhaps of diaphoresis. In the one, pain is the chief symptom; in the other there is discomfort rather than acute pain, and great excess of some secretion. The two hardly ever go together. A catarrh will cure a sick headache.

to the recognition of the essential nature. Of remedies which are capable of preventing the attacks of these recurring maladies I believe arsenic takes the first place. My facts, however, by no means justify a very positive statement to this effect. In some cases it most undoubtedly does so. In the case of a boy, whose portrait is preserved in the Clinical Museum and to whose case I have alluded on many occasions, arsenic appeared to be specific. It invariably caused the disappearance of the erythematous patches, and, under continued resort to it (with long interruptions), the liability at length ceased. Evidence of a similar but less conclusive character I have obtained in many similar cases. Thus we appear definitely to bring these curiously recurrent and periodic eruptions into alliance with recurrent herpes, which also is certainly amenable to long courses of arsenic. I believe that arsenic not only prevents recurrences, but that it shortens the duration of an attack if given early in its development.

CASE I.—*Erythema Multiforme arrested by Arsenic.*

The case of a young lady, a governess, whom I produced at one of the Clinical Demonstrations recently, seemed to be an illustration of this. I prescribed arsenic for her on a Friday, when the eruption was coming out, and arranged for her to attend on the following Tuesday. She did so, but, to her surprise, had but little left to show. She had experienced many previous attacks and knew their usual duration.

CASE II.—*Cheiro-pompholyx frequently recurrent during eight years—Feet affected on one occasion—Patient a healthy girl—Cure by Arsenic continued during two years.*

A case, for the opportunity of seeing which I was indebted to Sir William Broadbent, may form a suitable companion to the preceding. In it we had an eruption of similar character, and with parallel tendencies to spontaneous decline and recurrence, but with a great difference as to frequency of the attacks. In Sir W. Broadbent's patient

the hands, feet, and face were the only parts affected, the trunk and upper parts of limbs being always free. The patient was, as in the preceding cases, in excellent health. I saw her first on December 19, 1893.

Miss M—— was a rosy-cheeked girl of fourteen, but looking older. She was the youngest of nine. She had been for about eight years liable to an eruption of vesicles on her hands (palms and sides of fingers), which would come and go. Her attacks would last from ten days to two or even three weeks, and for the time quite disabled her hands. The intervals would be six weeks or two, or even three, months. The longer the interval the worse usually the attack which followed it. During the intervals the skin became perfectly sound. It always peeled after an attack. The attacks were often attended by hot and irritable lips, but no definite vesications ever formed on them. It had been hoped that the establishment of menstruation would end the liability, but it had not done so. The hands were always affected, but on one occasion spots had appeared on the feet, and on one other the face had been swollen and puffy. There was not the slightest tendency to neurotic ailments to be traced in the patient. She was robust and vigorous, and accustomed to an out-of-door life at her home in Scotland. The two hands were said to be always affected simultaneously, but not always with quite equal severity. The vesicles came out abundantly in the palms and between the fingers, but never on the backs of the hands. Her mother said that on one occasion a vesication formed of such size that it looked as if there had been a scald. No premonitory symptoms had ever been noticed. The girl was not catarrhal, and did not know what it was "to catch a cold." A sister who came with her spoke to a most definite difference from herself in this matter.

When I saw Miss M—— her palms and inter-digital spaces were affected as usual. The vesicles and little bullæ were all of them rounded or of beehive form, and contained clear fluid. They were abruptly defined and there was no congestion of the adjacent skin.

Arsenic was, of course, the remedy which was prescribed, and I believe it may be recorded that it succeeded in curing

the disease. I have not seen the patient for more than a year, but on the last occasion she was quite well, and had been so for some time without any threatening of recurrence. The following notes record her progress.

December 19, 1894.—She has taken Pearson's solution of arsenic in two-minim doses regularly. The eruption soon disappeared. In the middle of January she had a relapse, but it was not severe. From that time until three weeks ago she has been free, but the hands then became hot and a little swollen, and a rather sharp attack followed. It began as little blisters, which joined and formed large blebs. At present the palms are peeling in large flakes. The eruption came between the fingers and in the palms, but not on the backs of hands. No cause can be assigned for the recent recurrence, but it has been suspected that sweets sometimes cause them. The health has been good, and she has never had "a cold." The dose of arsenic is to be increased to four minims.

September 21, 1895.—I last saw Miss M—— in May, when she had a slight attack. She is now quite well. She has since May taken four minims of liquor arsenicalis regularly three times a day. She has been in good health and has had no attack. She does not now feel so sleepy as she used to do. It has been observed that eating sweet fruits will sometimes bring out a few little blisters. She menstruates regularly.

It will be seen that this case affords an excellent example of what many years ago I ventured to designate Cheiro-pompholyx. The case which I published with a portrait in my Illustrations of Clinical Surgery was indeed almost an exact counterpart of it, excepting as to severity. In both the eruption was restricted almost wholly to the hands, vesicles having been noticed on the feet on one occasion only in each. The transitory nature of the eruption, the completeness of its disappearance and the frequency of its return were alike in the two. One of the patients, however, was a very neurotic lady in middle life, and the other a perfectly healthy child of six.

I may here add that in my definition of "cheiro-pompholyx" the clinical history always took a part. It was a necessary feature that the disease should be almost wholly restricted to the hands and feet, that it should show no tendency to travel up the fore-arms or to pass into eczematous conditions, that it

should be of limited duration and prone to recur. It is in my experience a very rare malady, and several of the cases which were published by other observers as illustrative of its microscopic anatomy were, in my opinion, not examples of the disease at all.

(To be continued.)

West, Newman chromo.

SYPHILIS.

No. LXXII.—*Deafness and Facial Paralysis in the secondary stage of Syphilis—Recovery from the Facial Palsy, and in one ear from the deafness—Permanent and absolute Deafness in one Ear.*

A gentleman, aged 27, had complete syphilis in the early part of 1895. He took mercury for nearly six months. In November of the same year, after the treatment had been left off for some months, he began to lose hearing in the left ear, and at the same time felt out of health. He was seen by a surgeon on December 2nd, who found the hearing impaired in both ears, and that with the left the watch could be heard only at half an inch. Four days later the deafness was absolute, and the facial nerve paralysed. He had been vomiting through the night, and his gait was unsteady. The membrana tympani showed nothing abnormal. The right ear was increasingly deaf also. Mercurial treatment by inunction was practised. At one time the deafness was such that he was obliged to use a slate to communicate with his friends. By December 25th, however, his right ear had so far recovered that he could hear a tuning-fork at six inches. It was noticed that he could hear better first thing in the morning than later in the day, and better in a room than in the open air.

I have not myself seen the patient whose case is described in the above notes. He resides abroad, and I have been consulted only by letter. The notes imply but do not definitely state that at one time the right facial nerve also was paralysed. The treatment by mercury and iodides (with continued slight ptyalism) had been so effectual that the motor power in both cheeks had almost completely returned. On the left side the paralysis had been absolute in the whole

distribution, and the recovery had been so far complete, that only the lower part of the cheek remained weak, and nothing was observable except during active movements of the part. Hearing had been fairly regained in the right ear, but the left continued absolutely deaf.

This case fits well with what I have recorded of several others, in which deafness with facial paralysis occurred during syphilis. In all the affection occurred early in the disease, and symmetry was threatened, if not fully attained, the ear symptoms developing with great rapidity. When facial paralysis attends the deafness (very rare) it comes on after the latter, and it disappears under treatment much more satisfactorily. I have never myself attended a case in which the facial paralysis was permanent, nor have I ever seen a case in the early stage in which it was double. A very remarkable example of absolute deafness in both ears, with permanent and symmetrical facial paralysis, will be found in ARCHIVES, Vol. VI. page 268. In this instance there was little doubt that the affection was syphilitic, but the history as regards dates could not be obtained.

No. LXXIII.—*Syphilis—An acute febrile eruption diagnosed as Scarlet Fever—Deafness and Optic Neuritis in the tenth month.*

A. E——, aged 27, had a chancre in July. It was followed by secondary symptoms, and in September following he had severe illness and was in bed for a week with very severe headache, high temperatures (105°), and general erythematous eruption, which was diagnosed as scarlet fever. No one else in the house, however, took it. He had severe headache and some delirium. His skin afterwards peeled. He described his urine as having at one time looked like porter. After this illness he remained for some time very weak. During this illness his syphilitic eruption and sore persisted. The mercurial treatment was interrupted, but resumed a month or two later. In the following June he began to be deaf, with a stuffy feeling and whirring noise in his head.

I saw Mr. E—— for the first time in December, when he was in fair health, but still had a symmetrical papular eruption, in some regions becoming lupoid. His right ear was quite deaf, and a whirring noise was always present. He said that "to sit quiet was misery, as the noise was always going on." It was still worse when the ear was covered, as on the pillow. He did not hear the watch on mastoid, and on the forehead he heard it only in the left ear. There had been some threatening of noises in the left ear, but he hears quite well.

Right optic disc red and with a skin of new vessels in it (?). Knee jump good.

He complained that in walking his right leg would "slip out," and he would stagger as if intoxicated. He would lurch into people he walked with. He once, when shaving, fell down. It was always with a feeling of failure in the leg that he reeled or slipped.

No. LXXIV.—*Complete Syphilis treated by Mercury for three months—Apparent cure—Good health for three years—Marriage—A healthy child—Recurrence of induration in the site of chancre in the fourth year.*

1890	23	September: A hard chancre on skin of penis, followed by eruption and sore throat. He took mercury to beginning of December. In December in hospital with abscess in liver from dysentery (Bombay).
1891	24	Successful opening into the liver abscess. In July he came to England and consulted me. No symptoms of syphilis then extant. No further treatment advised.
1892	25	Quite well. Living in India.
1893	26	Married in February. Both himself and his wife remained in good health.
1894	27	On July 17th an indurated chancre recurred in the site of the original one. It disappeared under mercury in three weeks. No fresh exposure had occurred. His wife was confined August 17th. Both wife and child remained well.

In the case above scheduled I did not myself see either the original or the recurred chancre. Both were, however, seen by competent medical men, and about the facts there can be no doubt. We have to trust to the patient's testimony that he had not on the second occasion incurred any fresh exposure. He is, however, strongly corroborated by the circumstance that the recurred sore was exactly on the site of the former one, and that too an exceptional one—the skin of the penis near its root. It is improbable in the highest degree that a man should twice receive primary infection in this part and exactly at the same spot. The prompt disappearance of the second sore under treatment and the absence of secondary symptoms are also facts which support the belief that it was a recurred induration and not a new infection.

We have then an instance of recurrence of induration in a chancre-site three years and a half after cure of the first. The patient had in the interval enjoyed good health and needed no treatment. He had also become the husband of a woman who suffered nothing and the father of a child which showed no taint. Such facts favour the belief that the phenomena of the recurred chancre are in the main local, if not altogether so, and that they do not imply the continued existence of the virus in the blood.

No. LXXV.—*Recurrence of induration in the site of a former chancre—Remarkable resistance to mercurial treatment; but final cure during salivation.*

A very remarkable example of a recurred induration which long resisted treatment occurred in the person of a Mr. T——. He first consulted me on April 5, 1888, being then thirty-five years old. It was eleven years since an attack of syphilis, and his motive for consulting me was that he was liable to herpes on the skin of the penis three times a year. In July of the same year, during my absence, he saw my son, who noted an induration in the site of the former chancre. Although Mr. T—— confessed to having exposed himself to risk six weeks before, the diagnosis was

"a recurred induration," and iodide of potassium was prescribed. The induration was in the frœnum.

I saw Mr. T—— on August 13th, when all inflammation was gone and the sore quite healed. It was, however, still indurated. He had some eruption which was probably due to the iodide. There had been no other secondary symptoms. As I could not feel certain that the sore was not from a second contagion, I ordered mercury. At the same time I insisted that he should not marry.

On September 21st all induration was gone, and no secondary symptoms had shown themselves. He begged hard to be allowed to marry, as he had been long engaged and the day was fixed. I did not see him again for a year.

In October, 1889, he called on me and said that he had married, and that no constitutional symptoms whatever had shown themselves in consequence of the supposed re-inoculation. After the last date I did not see Mr. T—— until May, 1893, when he came to me with a very large and exceedingly hard chancre in the prepuce just to the right of the frœnum. It had been present, he said, for several months, and had given but little inconvenience. His wife was advanced in her fourth pregnancy. They had three healthy children living, and his wife herself was quite well. On this occasion he denied any fresh exposure. The chancre was in the form of a collar, and as hard as cartilage. It presented ulceration, without secretion, in two spots. It was almost exactly in the site of the original sore.

From this date I had Mr. T—— under treatment for nearly a year. The iodides and mercury in various forms were given very freely, but for a long time it seemed impossible to make any impression on the induration. Mercury was used both by inunction and internally, and at one time, in February, 1894, my notes state that he was salivated. At this date the induration was very much less, but it had not wholly disappeared. During the next two months it continued slowly to diminish and soften, and in May, 1894, I was able to record that it had quite disappeared. There had never been the slightest tendency to sore throat or eruption. Appended is schedule of his case.

YEAR.	AGE.	DETAILS.
1877	24	Contracted syphilis.
1878	25	Under treatment.
1879	26	Liable to recurring herpes in penis, but no other symptoms.
1880	27	
1881	28	
1882	29	
1883	30	
1884	31	
1885	32	
1886	33	
1887	34	
1888	35	Induration in site of former chancre; second contagion possible.
1889	36	Married; no constitutional symptoms.
1890	37	Wife well, and three healthy children.
1891	38	
1892	39	
1893	40	Came to me with another induration in same place.
1894	41	Salivated; induration cured only after long treatment.
1895	42	Well.

No. LXXVI.—*Protracted Syphilis—Long treatment—Marriage—Single patch of Lupus.*

Mr. T. P. C—— has been under my observation at intervals for fifteen years. In the first instance he suffered severely from syphilis, the early treatment of which had been neglected. Six years ago I allowed him to marry, and he has now a child, aged 4, in excellent health: nor has his wife ailed anything. A year ago, however, he came to me for a small lupoid patch on the side of his abdomen, for which I gave mercury. To-day (April 1, 1895) he comes with a very curious little patch on the skin of his penis, which has caused him great alarm, but which was probably of no consequence. The lupus patch, above mentioned, has left a depressed scar. It was cured by specifics, and I do not think there can be any doubt that it was a tertiary phenomenon. Mr. C—— is otherwise in excellent health. In the earlier stages of his syphilis he had very troublesome reminders, an ulcerating herpetiform eruption on his forehead, etc., which had long resisted treatment.

No. LXXVII.—*A case illustrating the rapid development of the Indurated Chancre.*

On Tuesday, Jan. 21st, a patient consulted me in great fear that he had a chancre. It was nearly a month since exposure to risk, and gonorrhœa had made its appearance a few days afterwards. He had been under medical observation ever since, and had himself carefully inspected the organ. Not the slightest abrasion had been noticed until the day before he called on me, when he found a little redness and irritation in the coronal sulcus in the left side. On careful inspection I found two little ulcers very near together, and surrounded by congestion with a little moisture. I told him that I hoped it was only herpes, but as the dates fitted with the diagnosis of chancre we must defer a positive opinion. He came to me again three days later, and I then found a collar of most characteristic induration, so that it stood up in an elevated roll, easily appreciated by the eye. The two little sores had joined, and the congestion was increased. On the opposite side of the coronal sulcus another little ulcer was commencing in exactly the same way as the first. I now told him that there could be no doubt as to diagnosis, and prescribed mercury.

No. LXXVIII.—*A very peculiar form of Eruption six months after a second chancre, and twelve years after a complete attack of Syphilis.*

I have never to my recollection seen any eruption, whether syphilitic or otherwise, exactly like what I am about to describe. It is not very uncommon in various stages of constitutional syphilis to recognise little shotty subcutaneous indurations on different parts of the limbs and body. They are, however, usually discovered only by the finger, and are wholly unattended by discoloration of the overlying skin. Sometimes these little lumps, which I have always supposed to be seated on or in lymphatic trunks, inflame and become surrounded by diffuse swelling which embeds them. In this stage, which occurs for the

most part on the legs, there may be congestion of the skin and adhesion to it, and ulceration may even occur. When this is the case we have an exact simulation of Bazin's malady. The cases are exactly alike, whether they occur with syphilis or independently of it, and there is little or nothing but the history upon which to base a diagnosis. The tendency of these lymphatic indurations to inflame, become erythematous, and ulcerate, is, as far as I have observed, almost confined to the lower extremities. In the case which I have now to describe, these little indurations formed in great abundance on all the four limbs, and almost all of them were attended by dusky venous congestion of the skin, which made them very conspicuous. None of them showed any tendency to ulcerate.

The patient was an officer in the Indian army, aged 38. He had suffered from syphilis (complete) twelve years ago, from which, under treatment, he had recovered. He thought, however, that he had been liable ever since to the occasional formation of little hard knots under the skin, but he had given them no special attention. Towards the end of 1893 he had another sore on his penis, which he supposed to be from infection, but which did not last long. It was followed, however, by large circinate patches on the forearms and thighs, like a very superficial form of lupus. For these, after they had lasted four months, he consulted me on May 8, 1894. I found that, in addition to the lupoid rings, he had some curiously livid blotches on his left hip. There were a dozen or more of them, all discrete but in a wide group. I told him that I thought he must have got a second infection, and prescribed bichloride with iodide. In the course of a few weeks under this treatment the rings wholly disappeared, without leaving any definite scars. The blotches, however, instead of disappearing, increased in number, and at the date of his next visit I found that there was beneath each a little shot-like induration. Towards the end of May his lower extremities, from buttocks to ankles, were covered with them, and in addition to the spots which were dusky, many little indurations were to be found over which there was no erythema. These were in

their first stage very tender to the touch, but the tenderness ceased when they became adherent to the skin and assumed the dusky congestion. There were a few similar nodules and spots on the upper extremities. Captain H—— assured me that the indurations were exactly such as he had noticed in his arms years ago, and repeatedly, only they had never before become numerous or been rendered visible by congestion.

A few weeks later, and in spite of the continued use of specifics in increased doses, the arms as well as the lower extremities were covered by these blotches. Captain H—— was throughout in excellent health, and had no other signs of syphilis. It was distinctly a limb eruption, his trunk being exempt. My last note was on June 11th, and the eruption then still persisted, but without any tendency to ulcerate or to change its character.

THE NERVOUS SYSTEM (HERPES, ETC.).

No. LXXII.—*On the possible influence of the Nervous System in localising the lesions of Inherited Syphilis.*

I have repeatedly asked attention to certain curious and very unexpected facts which would seem to imply that the nervous system takes some share in the localisation of the later lesions of inherited syphilis. The chief fact of this kind is an occasional want of symmetry in the notching of the incisor teeth. I possess several drawings, one at least of which has been published, which illustrate this. We have further the probability to which also I have adverted, although I did not recognise it in the first instance, that the malformation of these teeth is really an arrest of development and not the direct result of inflammatory action. In this feature it stands in very definite contrast with the malformations which are, I think, attributable to infantile stomatitis, the so-called mercurial teeth. This failure of bilateral symmetry occurs not only in the incisor teeth, but occasionally also in the keratitis, in the deafness, and even in the synovitis, which are such characteristic phenomena of the later stage in the subjects of this taint. Many years ago I had under care a patient who had only one notched tooth, and in whom the keratitis, contrary to almost invariable rule, affected only one eye, the malformed tooth and the keratitis being on the same side. It is difficult to explain such facts without having recourse to the influence of the nervous system on nutrition. It is to be admitted, however, that they are as yet but very few in number, and on this account possibly not much importance ought to be attached to

them. They derive, however, some little support from the fact that it is exceedingly difficult, without recognising some law which we do not understand, to explain the remarkable delayal to late periods of the inflammations just referred to. Why a patient, the subject of taint, should enjoy apparently good health up to the age of ten, twenty, or even thirty years, and then pass through an acute and well-marked attack of keratitis it is exceedingly difficult, under any of the known laws of pathology, to explain. Neither the otitis which causes deafness, nor the synovitis which often accompanies the keratitis are, I think, ever delayed through such long periods as we now and then observe in the case of the latter. But this does not materially lessen the difficulty of explaining them, for they hardly ever occur before the age of six, and are usually later still. The very remarkable fact that each one of the affections alluded to comes under a law of spontaneous subsidence, is also in itself of some weight in support of the theory which I am suggesting. The late phenomena of acquired syphilis, periostitis, gummata, lupoid affections of the skin, &c., show no tendency to spontaneous decline; rather they persist indefinitely and become aggravated if specifics be not used. Now, although the otitis of inherited syphilis very usually destroys the function of the organ, and in exceptional cases the keratitis may do the same, yet it is most certain that both these affections are amenable to a law of spontaneous decline. In each instance the organ, however much it may have been irretrievably damaged, passes into a state of quietude and is no longer liable to any relapses. The duration of the attacks is, indeed, capable of being averaged with no very wide limits of difference. Again, we may remark that it is most difficult on any theory of blood-poisoning alone to explain such facts, whilst they fit well with what we know as to the maladies which are induced through the agency of the nervous system.

My attention has been recently again drawn to the speculations stated above, by several cases which have come under my observation.

About nine years ago I attended a man and his wife, then

recently married, for constitutional syphilis. I have been unable to find my notes of their cases, but I believe that I did not see either of them until some time after the original disease. From the woman I received a portion of alveolus which had undergone necrosis. Both man and woman have enjoyed excellent health ever since the treatment. I have seen them both quite recently. A child was born about the time that they were under my care, who suffered mildly from infantile symptoms, but who is now a well-grown, robust-looking girl, nine years of age. This child is florid, and shows no peculiarities of physiognomy, excepting that she has some deep scars at the left corner of her mouth. Both eyes have recently passed through an attack of keratitis, but the left suffered much more severely than the other, and has been left somewhat damaged, whilst the other has quite recovered. Just as the keratitis was subsiding, she had free effusion into the left knee joint, the right remaining quite free from swelling, although, as she said, aching a little. Thus it will be seen that the deviation from bilateral symmetry is of the same kind in respect to all the lesions. In all the disease has been most severe on the left side, although not quite absent on the other.

In suggesting that the nervous system is probably the agent which determines deviations from symmetry in all cases, we must not forget that it may possibly often play only a secondary part. It may itself be influenced by a poison which has at some former period had its existence in the blood. The best instance which can be alleged in illustration of this is the herpes which follows the administration of arsenic. Here the drug circulates freely in the blood, but the resulting phenomenon is unilateral and clearly due to the nervous system. We must prepare our minds for the discovery of some laws in respect to these facts which we do not as yet understand.

No. LXXIII.—*On recurring Herpetic Inflammations after Syphilis.*

The influence of syphilis in producing local forms of herpes which are very prone to recur is most remarkable.

It has claimed the attention of many observers, more especially, I think, of the French. No sooner has a man had a chancre than he becomes liable to herpes of the prepuce. It is said that a non-infecting sore, or even a gonorrhœa, will sometimes have the same influence. This may be so, but if I were to trust my own experience, I should believe that this kind of herpes is not common excepting after infecting chancres. After these it may begin within a few months, and may continue to recur for many years. I have just seen a man whom I had treated about eight months ago for a hard chancre, and who was still taking mercury, and upon whose penis a large group of herpetic vesicles appeared. It was not merely preputial herpes, though some were under the prepuce, for a long group of vesicles ran up the skin of the penis.

Nor is it only on the genital organs that we encounter recurring herpes after syphilis. Many of the troublesome affections of the throat, lips, and mouth are, I am convinced, herpetic in character. Their real nature is shown by the grouping of a number of little round sores near each other, and on one side only, by their sudden occurrence, and often by their spontaneous decline and liability to recur. They are usually considered both by the patient and his surgeon as directly specific, and as discouraging relapses. As a rule, however, arsenic is much more efficient in preventing them than either mercury or the iodides. The soft palate and the pillars of the fauces are parts upon which herpetic ulcers are often seen, but they may occur anywhere. The soreness and pain which patients often complain of low down in the throat, and possibly some of the laryngeal affections also, are in reality herpetic in nature.

The syphilitic forms of herpes present some peculiarities. They do not always subside so quickly and completely as other forms, and may even occasionally pass on into persisting sores. This is, however, rare, and probably occurs only in cases in which there is blood poisoning still extant as well as nerve disturbance. In such cases a mixed treatment of arsenic with mercury and iodides is demanded. As

a rule specifics do nothing for the post-syphilis forms of herpes, and there is no reason to believe that they imply persisting blood infection. Many men who are married and have healthy children remain still liable to these forms of herpes.

It is perhaps not improbable that a local inflammation has as much to do with producing herpes after syphilis (a form of peripheral neuritis) as the blood contamination itself. Thus the chancre induces it on the penis, and the sore throat evokes it in the palate. This, however, is little more than conjecture.

No. LXXIV.—*On the Doctrine of Undeveloped Herpes.*

It has been unfortunate for herpes as with many other diseases of the skin, that it has received its definition and description from external appearances rather than from any recognition of its real nature. It is regarded as essential to the conception of herpes that it should be a vesicular disease. Its real definition ought to be, as was long ago suggested, a local neuritis which is sometimes attended by vesication over the area of the nerve affected. Probably if we could know the truth, the number of cases in which vesications do not appear, is much larger than that in which they are present. The vesication cases (common shingles, and the like) may be reasonably supposed to be only the more severe type of the malady. It is very desirable that we should recognise clearly that the nervous system is liable to well-localised forms of peripheral neuritis, which may be attended in some cases only by a little heat, tingling and soreness, and in others by the formation of grouped vesicles over the end-organs, or even of gangrene of the affected portions of skin. The vesicular cases, although relatively severe, are not the worst type, for, as is well known, there is a form of herpes in which, instead of a vesication, the portion of skin involved passes at once into gangrene.

As regards cases of "undeveloped herpes," it is probable that almost the whole of them pass without recognition. The patient complains of pain or soreness over one half of the

scalp. He is examined and nothing is found; and as there is no material constitutional disturbance the case is dismissed as one of neuralgia or of rheumatic pain, and as it passes away in the course of a few days, nothing more is thought of it. Such cases of abortive herpes are not only, I believe, very common, but may affect the most various parts, such as the tongue, palate, throat, and any of the parts of the skin ordinarily affected by more typical herpes. The cases are to be recognised by their being definitely confined to one side, and often to the region of some known nerve. The patient will sometimes assist the diagnosis by distinctly stating that he can feel with the hand, although there is no perceptible redness and nothing that any one else can detect, a number of little points in the skin, which are not only very tender but slightly hard. The doctrine itself receives strong support from the recognition of half-way cases. By these I mean cases in which an eruption is threatened by local congestion and pain, but never developes vesicles. There are papular forms of herpes which are quite definite. They are rare, but those who have seen them can feel no doubt as to their reality.

No. LXXV.—*Herpes recurrent at definite intervals.*

An interesting example of the periodicity of recurring herpes was presented by a gentleman named L——. He was thirty-six years of age. Fifteen years before I saw him he had a chancre, but so far as he remembers he had no particular treatment, and no constitutional symptoms followed. Ever since, however, he has been liable to herpes on the penis. At first it came several times in the year, but latterly it has settled down to a regular spring and autumn recurrence. It was his druggist who drew his attention to this periodicity, remarking, "You always come to me in April and November." On referring to his books he found that these dates had been very accurately observed for several years. Mr. L—— knew of no special influence which caused his herpes. It had nothing to do with sexual intercourse, and he had been married ten years. His

chancre, which had been treated by caustics, had left a scar which was still conspicuous when I saw him. He was in excellent health, but the recurring herpes caused him great annoyance.

No. LXXVI.—*Herpes of the Occipital Nerve, with Paralysis of the Palate and Pharynx on the same side.*

A very important case of herpes in which motor and sensory paralysis were associated with the eruption, was described to me by an intelligent patient. I did not see him during his attack, but he told me that his case had excited great interest in the medical men who attended him. According to the patient's description he had developed an herpetic eruption on the right side of the scalp on its posterior half. He described it as having extended forwards to exactly the part where herpes frontalis usually ends. It was attended by much pain in the back of the neck. Following very quickly on the eruption there was what he called paralysis of his throat. He had great difficulty in swallowing; the uvula and soft palate were motionless on the paralysed side, and moved towards the opposite one when irritated. At the same time the left half of his tongue and palate had entirely lost sensation. In the course of a few weeks he recovered, but at the time I saw him, some months later, the uvula still diverged very definitely towards the unaffected side. The herpes had not left any visible scars, nor did he complain of numbness or pain in the skin. The case had been seen by Dr. Manley and Dr. Nelson, of Belfast, and subsequently by Dr. Semon in London.

Dr. Manley was subsequently kind enough to supply me from memory with the following narrative, which supplements what I had obtained from the patient himself.

"WHITEHOUSE, BELFAST, *March* 22, 1894.

"DEAR SIR,—As you seem to be interested in a severe illness he had about two years ago, in which herpes formed a prominent symptom, I shall try and give you from notes a short history of his case. On August 12, 1892, Mr. B——, after returning from a day's shooting, threw himself

on the grass in the garden, and while lying there had some tea. The next day he complained of neuralgia on the right side of the head and face. The day following (14th) there was a distinct herpetic eruption on the right side of scalp and neck, also over right side of palate and fauces. The next symptom to present itself was partial aphonia, succeeded by difficulty in swallowing, the food, if at all solid, getting, as he described it, into a 'point' in the œsophagus, or else returning through the nose. Mr. B—— found that this tendency for the food to stick was partially obviated if he lay on the right side. There was also slightly diminished sensation on the right half of tongue, and though the palate was drawn to the left side, yet the tongue was protruded in the central line. He was not able to whistle as usual. He was examined by Dr. —— (throat specialist), who found paralysis of the right vocal cord, and declared the disease to be most likely central. I differed from this opinion, and looked upon the ailment as a peripheral neuritis. He also suffered from more or less paralysis of trapezius and low angul scapular muscle on same side. His improvement was most gradual, but he completely recovered. I regret I cannot supply you with more copious notes of this most interesting case.

"I am, yours truly,
"H. C. MANLEY."

VACCINATION AND VARIOLA.

The Initial Eruption in Variola.

Variola, like syphilis, has its initial eruptions, which are different from its characteristic one, and usually disappear when the latter commences. An excellent *résumé* of information respecting them, with some illustrative cases and diagrams showing location, will be found in Vol. X. of the St. Thomas's Hospital Reports from the pen of Dr. Sharkey. These rashes may be simply local erythema, may resemble measles or scarlatina, or may be petechial or hæmorrhagic.

Vaccination as a cause of Increase of Cancer.

The opponents of vaccination have attempted to make use of the fact that cancer is on the increase as a reason for believing that it is to some extent attributable to it. No doubt it is, but not exactly in the way that they wish to believe. Vaccination has immensely reduced the number of those who die in early life from small-pox, and it follows that it must have enabled a larger number to grow up to adult age. To realise this we have but to imagine that all the diseases of childhood were suppressed as completely as small-pox. It is clear that with the reduction of child mortality must be associated a corresponding increase in that of aduits. With the increase of deaths in adult and senile periods will come of necessity an increase in the deaths from cancer.

Small-pox in former times.

The following facts, which I extract from an Essay on Leprosy, may perhaps be read with interest. They present

us with a vivid picture of the realities of small-pox in pre-vaccination times:—

"An Icelander had been abroad, and died during the winter from small-pox. In spring his clothes were brought on shore at Örebakke, and among these a chest filled with linen. His sister put on one of his shirts, and contracted small-pox; in this way the disease spread all over the country. Thirty-four years had elapsed since the previous small-pox epidemic. Several old people who in former days had suffered from the disease caught it again, while most of the young persons, especially men, were attacked. Women were occasionally obliged to carry their dead to the church. The bishop had to saddle his horse himself, and to ride out alone. Twelve thousand persons are said to have died in the diocese of Skalholt, and 6,000 in the northern districts, making altogether 18,000, thus leaving only about 34,000 inhabitants. The majority of the lepers died on this occasion, and, besides these, most of the old persons and the pregnant women."

The Relationship of Erysipelas to Phlegmonous and Ulcerative Inflammations.

Important evidence as to the identity of the poison which produces cutaneous erysipelas and ulcerative inflammations with abscesses may be obtained from the narratives of epidemics in connection with vaccination. In a series of which the record is before me (Appendix, p. 238) fourteen children suffered and two died. In one of these the arm inflamed within twelve hours of the vaccination, and subsequently erysipelas spread over the whole body with great œdema, but no abscesses. The child recovered. This was the only one of the series which had typical erysipelas, but several others had inflamed arms, with much swelling and ulceration of the sores. Several had glandular suppurative and diffuse abscesses in cellular tissue (p. 235).

THERAPEUTICS AND DIET.

Salivation not an indication for the disuse of Mercury.

I feel sure that we make a mistake when we speak of salivation as being proof of the full influence of mercury. It is rather like diarrhœa—a collateral inconvenience, and nothing more. It does not by any means prove that further use of the drug will be of no advantage to the patient. There are many cases in which in spite of salivation it is essential to push mercury. Several important illustrations of this observation have recently come under my notice. In one of these a young man was sent up from the country in an extreme condition of cachexia, from secondary syphilis imperfectly treated. He was emaciated, anæmic, and had several large ill-conditioned ulcers in his limbs and body. He was under the influence of mercury, his gums being swollen and sore, and his breath having a strong mercurial fœtor. It was suggested that it was a case for cod-liver oil and tonics rather than for specifics; but as the ulcers were unquestionably specific, and as it was only eighteen months since the primary sore, I ventured to overrule this and to insist on more mercury. A careful local treatment of the mouth was at once commenced, and small quantities of mercury by inunction were continued. The patient was at the same time allowed to keep his bed and encouraged to take a liberal diet, with small doses of quinine. The result was that he rapidly began to improve, and that in the course of six weeks he had gained a stone and a half in weight, and that all the ulcers had healed.

Severe Urticaria after eating high game—Subsequent attack of Jaundice.

A vigorous and healthy young man ate on a Saturday a cold partridge which was very high. He was aware the next day that it had disagreed with him, for he felt sick and had no appetite. This continued until on the third day urticaria appeared. On the tenth day he was brought to me covered from head to foot with large wheals. These wheals had, he said, varied in place somewhat, and had continued to enlarge at their borders. Some of them were as large as the palm of an adult hand. All were of a salmon tint. Although the tongue was quite clean there was entire loss of appetite. At this date I did not notice any jaundice, but when five days later the patient came to me again he brought a bottle of his urine, which was deeply coloured with bile, and he was himself very yellow. His urticaria was now fading. A week later, under salines and mercurials, the jaundice was almost gone and the appetite was returning.

The evidence connecting the attack with the too high game seemed satisfactory, for although there had been an interval of a few days, yet the patient had felt out of sorts all the time. It may be a question whether the urticaria was due to bile in the blood, or to some ptomaine from the decomposing game.

Notes on Tea, Coffee, and Wine.

A surgical friend, accustomed to do operations on the eye told me that he could never take coffee in the morning because it made his hands perspire. A like result would follow marmalade; but that it was not due to the sugar seemed proved since he could take sugar in tea with impunity. He was accustomed to take coffee later in the day without inconvenience.

Another friend has told me that he never takes tea at breakfast when he is going to shoot. It makes him, he says, rather nervous and prone to shoot too quickly. Coffee, on the other hand, gives him nerve.

Another sporting friend, a physician, who is fond of drugs,

always takes a does of ergot of rye before a day in the fields. He thinks that it improves his sight and enables the eye and hand to act together.

The practice of mixing some form of spirit with tea and coffee is, I believe, one based on experience. The sinking at the pit of the stomach and sense of want which often follow a few hours after tea or coffee is always relieved by an alcoholic stimulant, and it is probable that it is often prevented. Cognac is sometimes called "French cream." In Holland, rum is brought with coffee for a like purpose. Probably the nervous system is never in a state of greater efficiency than after a cup of coffee "laced" by a tablespoonful of rum or brandy. The Scotch peasant puts whisky into her tea and with the knowledge that the ill-effects of both will be prevented by the mixing.

It is not so well known as it might be that tea and claret go very well together. They may be taken either at the same meal or within a short time of each other, and have the effect of preventing disagreement from either. The claret will not cause headache, and the tea will not cause nervous depression. The poorer the quality of the claret, the more helpful is the combination with tea. Coffee does not suit claret so well. After a claret dinner, tea should be taken; but after champagne, coffee. Tea and champagne do not agree well together.

Champagne in many persons, if taken continuously, has a decided effect in producing pruritus ani, more especially if there are piles. In some it causes secretion from the glands around the anus and makes the part feel disagreeably moist. It is well in all affections of the anus to forbid champagne, whatever may be its quality.

Some wines, to some persons, are distinctly aperient. Madeira of the very best quality will not unfrequently purge, and is, I think, always laxative. In this respect, as in others, it differs widely from sherry. The latter, like brandy, almost always constipates. The Hungarian white wines, more particularly Œdenberg, are aperient, and will now and then purge severely. Wines that are laxative usually agree well with bilious persons.

Of all wines that are restorative in nervous exhaustion there is none so efficient as Madeira, unless it be champagne.

Claret and Burgundy are far inferior to champagne and Madeira in temporary states of exhaustion. Champagne is well known to be the debauchee's wine. It is a sexual stimulant and restorative. For blood-making purposes and for continued use, however, the red wines are infinitely preferable. Burgundy in particular is reputed to have a special effect on relaxed throats.

For nutritive qualities and to those who can digest them, no wines can compare with malt liquors; unfortunately many cannot digest them, and especially are they unsuited for a sedentary life. If it be wished to fatten, there is nothing like bottled stout. If it be desired to avoid nervousness and to get rid of insomnia, shun tea and coffee and drink Guiness' stout. At the London Hospital I always treated the delirium following wounds or other injuries by an unlimited allowance of beer, and had very remarkable success. I scarcely ever met with a man who could withstand the soporific effects of bottled stout. It is far better than opium and induces a more nearly natural sleep.

In the middle class of English society most men pass through set stages as regards their dietetic stimulants. In boyhood, nothing stronger than tea and coffee; in early manhood, beer; in middle age, claret; and next, after trying white wines, port, and sherry, we settle down at last on that wretched substitute for them all, whisky and water. Happy is he who can go on with beer all his life. In the present day many resign themselves to whisky far too early. In this, as in all other matters, *senectuti resistendum*—put off the evil day as long as possible.

The Uses of Strawberries.

Dr. Withering writes of strawberries: — "They are grateful, cooling, sub-acid, juicy, and have a delightful smell. They promote perspiration, and dissolve the tartarous encrustations upon the teeth. Persons afflicted with the gout or stone have found great relief from using them

largely; and Hoffman says he has known consumptive patients cured by them."

Withering's four-volume work on Botany is well worthy of a place in every medical man's library, for, apart from its special object, it is replete with remarks on medical and dietetic subjects. Its author, as all know, was a distinguished physician of wide learning. It was of him, when on his death-bed, that it was plaintively said that the flower of physic was withering.

On the addition of Cane Sugar to Wines.

The following extract from a letter from my friend Dr. Meynell Williams, late of York, contains, I think, a valuable hint:—

"YORK, *May*, 1875.—I learnt a fact the other day from a friend of mine that may be worth your notice, if not already aware of it. He is in the wine trade, and thoroughly understands the action and manufacture of wines. He is very liable to the gout, and said that if he was to drink ordinary sherry for a few days he would be sure to suffer from it, and I desired him to explain the reason of it to me. He stated that in the manufacture of sherry there was always a certain amount of saccharine matter added to the sherry originally abroad to suit it to the public palate. Now it is the addition of this saccharine matter, which is *cane sugar*, to the *grape sugar* already in the wine that does the mischief. He could drink any amount of wine not containing any *cane sugar* with impunity. Does the union of grape and cane sugar produce any peculiar action in the stomach which induces the gout by producing some particular acid reaction? I am not chemist enough to say, but I thought the fact might be new to you, and of some service in your present researches on gout and rheumatism. My friend attributes many dyspeptic attacks to the same cause. If you talk with any wine merchants on the subject you will find they understand nothing about it; they know what suits the palate, but are totally ignorant of any chemical facts. I asked if there was any way of detecting the presence of cane sugar chemically. He did not know that fact, but he had studied the taste of wines thoroughly, and could always detect it by the palate. All wines are to a certain extent fortified by spirit before importation. Now the wine may be free from cane sugar, but the spirits put to it contain cane sugar and produce the same effect. I tasted a sherry with no cane sugar in it, the spirit to fortify it being made from the sherry itself; it was not at all palatable, and you would have to become used to it before you could drink it with pleasure. Some clarets and hocks are entirely free from cane sugar, and therefore agree with some stomachs where nothing else is admissible."

MISCELLANEOUS.

No. CCXIX.—*On Laparotomy for Abdominal Obstruction.*

In the formation of opinion it is impossible to reject altogether the influence of hearsay, especially so in the absence of any trustworthy statistics. A house-surgeon has just now told me that in the hospital with which he is connected ten patients in succession have died after laparotomy operations for intestinal obstruction, most of them having survived only a few hours. On the morning of the day on which I write this, two patients have come under my notice whose statements offer items of evidence. One of these brought a letter from his surgeon stating that he had recovered from "acute abdominal obstruction with local peritonitis." The man was again in good health. In corroboration of his surgeon's statement, he gave me the history of a month's illness, which set in suddenly and severely, and at any stage of which an operation might seem to have been demanded. In the second case a patient apologised for not having kept an appointment, made some weeks before, by stating that he had had a very serious illness. He had, he said, been confined to bed "with obstruction of the bowels, and not expected to recover." I inquired in detail as to his symptoms, and found that he had really passed through an acute and severe attack such as might have well been supposed to justify, and even to demand, an operation. Yet he had made a good recovery.

No. CCXX.—*Successful Abdominal Taxis.*

Mr. Croly, of Dublin, in his interesting paper on Hernia to which I have referred at page 142, narrates the following

important case in which jactitation effected abdominal taxis under very urgent conditions.

"A most instructive and interesting case was mentioned to me by my late friend, Mr. Young, of Monaghan, surgeon to the county infirmary. A young man was admitted to his hospital, suffering from intestinal obstruction; all the usual treatment short of operation had been tried without effect—even to the swallowing of a large quantity of quicksilver. The friends at last came for their son, having an idea, and probably correctly, that a *post-mortem* examination was contemplated. No persuasion on Mr. Young's part, or that of his staff, had any effect in deterring the patient or his friends from the risks attendant on his removal to a distance in the country, and accordingly he was placed in a common cart without springs, and with straw for a bed. The patient was jolted home, and on his arrival was put to bed; shortly afterwards some of the young children in the house were seen on their hands and knees trying to *catch* the quicksilver, which had passed through the obstructed bowel. The result of this case was a perfect recovery."

No. CCXXI.—*Another Case.*

The following letter from my friend Dr. Frith, of Gravesend, needs no comment:—

"GRAVESEND, *June* 6, 1894.

"DEAR MR. HUTCHINSON,—I find I have the following brief notes of the case of abdominal obstruction I spoke about:

"A. B., a big girl, æt. 10 years, had always enjoyed good health, and had an excellent appetite, but suffered frequently from constipation. On the occasion of my seeing her, in consultation with her medical man, she had not had any action of the bowels for a fortnight; all aperients which had been given had failed, and now sickness had set in, and I was called in to see her with a view to the question of any operative measures. I found her in great pain, rather feverish, and flushed; the abdomen was distended and tympanitic, but not very tender, and there was constant sickness, the vomited matter being sour but not fœcal. We decided to try abdominal taxis, so we put her under chloroform, kneaded the abdomen, shook her up and down and from side to side, turned her on her side and kneaded again, and finally got her father to hold her up by the feet (for she was no slight weight), and in this position administered a large enema of hot soap and water. The injection was retained, but no result immediately followed; however, during the night she had an enormous relief from the bowels, and the motions of more than fourteen days all came together: the sickness stopped, the pain subsided, and in a day or two she was quite well.

"You can make what use you like of this case. I remember it occurred

soon after the first number of your ARCHIVES came out, and it was your strong advocacy of 'Abdominal Taxis' that led me to give it a trial; and I afterwards brought the case before the notice of our district of the B. M. A.

"Believe me, faithfully yours,
"CHARLES FRITH, M.D."

No. CCXXII.—*Racemose Glands in the prolabium of Upper Lip.*

On the prolabium of the upper lip there sometimes appears a row of enlarged glands. They form a broadish streak along the middle of the prolabium, not touching the edge of either skin or mucous membranes. They are minutely racemose and like the meibomian glands. The following is an illustrative case.

Mr. W——, æt. 22, who shows me this streak, has known of it for a year. They cause no soreness. There are none on the lower lip and none in the cheek pouch. The glands are of a yellowish-grey colour. They were sufficiently conspicuous to have attracted the patient's attention, and he asked me about them. When his mouth was shut they were scarcely to be seen.

These glands may be found in the lips of many persons if carefully looked for. It is very seldom, however, that they become conspicuous. It is much more difficult to find them in the prolabium of the lower lip.

No. CCXXIII.—*Statistics of Cancer in Australia.*

I mentioned in an earlier volume a statistical paper from New Zealand which asserted that deaths from cancer were rapidly on the increase there. My explanation of this was that the number of those in senile and pre-senile periods was likely under the known conditions of colonisation to have increased. The old do not usually emigrate, and consequently fifty years ago the New Zealand population was one which, as regards age, was not likely to supply many cancer subjects.

From Australia we now have statistics of another kind. It is endeavoured to show that the native-born population

suffer from cancer much less than the English-born. The same fallacy seems to underlie this assertion, and the two statements mutually support my explanation. In the present population of Australia the native-born must comprise a far larger proportion of children and young persons than does that of those born in the mother country. The latter will therefore supply a far larger proportion of those who have arrived at the cancer age than the former. It is fair to state that Dr. ——, the author of the paper to which I refer, does allude to this fallacy, but he does not, I think, by any means adequately meet it. I have often expressed my surprise that those who offer statistics of cancer in reference to race, climate, and place of residence, do not recognise that all calculations are worthless which do not afford complete information as to the relative numbers of those past middle age in the communities subjected to comparison. To my mind it wholly invalidates all the statements which I have read on this point.

No. CCXXIV.—*Bazin's Malady, a supplementary Note—Coincidence of Scirrhus of Breast with Scrofula.*

One of the most important of the cases which I have published in illustration of Bazin's malady is that of a lady, Mrs. W——. This lady has been under my observation for the ulcerations and indurations on her legs for fifteen years, and she is still not well. That she is strumous cannot be doubted, for she has the scars of gland-abscesses in her neck. Recently, however, she has developed scirrhus in the left breast, and I have excised the mamma, and six months later some glands from the armpit. Her legs are covered with scars, and still occasionally develope subcutaneous indurations of the most characteristic kind. These slowly break down into ulcers, unless cured by treatment. The local treatment which always suits is an ointment containing cinnabar and calomel to the sores, and over this an evaporating lead and spirit lotion. The latter appears to be efficient in dispersing the indurations.

We have in this case an instance of scrofulous and cancerous processes extant together. The older pathologists used to teach that there is an antagonism between tubercle and cancer, but certainly there are many exceptions to such an observation.

I have recently excised a breast for scirrhus, and on the same occasion removed from the opposite axilla a large mass of scrofulous glands. The patient was a married woman, aged 38, for whom I had on two previous occasions excised tuberculous glands. On this last occasion the glands were wholly transformed into caseous tumours. The scirrhus of the breast was well characterised both to the naked eye and the microscope.

No. CCXXV.—*On the importance of perseverance in inquiries as to the inheritance of Gout.*

It is always necessary to be very cautious in accepting the first statements of patients as to what they inherit. Especially is this the case in reference to gout. Some will willingly admit it upon the slightest evidence, but the majority will deny it if possible. The ideas of many as to laws of inheritance are very restricted. The following verbatim notes of a conversation may illustrate this.

A man of forty-eight consulted me on account, amongst other matters, of inability to kneel without pain. "I feel stiff in the knees; I cannot run, and the other day, having occasion to seek something on the carpet, I tried to kneel, and found that it gave pain."

Q. Do you inherit gout?

A. Not in the least. I am the youngest of thirteen children, all healthy, and my father lived to be eighty.

Q. Do you not see that your father might have had gout although he lived to be aged? Being the youngest is rather a reason why you should inherit it than otherwise. The older the father the more likely he is to be capable of transmitting gout.

A. Well, my eldest brother was twenty-six years older than me, and I have several nephews who are my seniors.

My father lived all his life very freely, and towards the end of his life often had gout in his great toe, but you see he had earned it.

Q. Then why did you tell me that you do not inherit gout?

A. I did not think that, as it was after I was born, it mattered.

As a pendant to the above I am tempted to relate another fragment of conversation which I once had with a not unintelligent patient.

Q. Have you ever had gout?

A. No, never.

Q. Have you ever had pain in your great toe?

A. I once had a bad inflammation in it.

Q. Was it painful?

A. Yes, very.

Q. What did it look like?

A. It was swollen and red and shiny.

Q. What did your doctor call it?

A. "Erysipelas."

Q. Did it spread up your leg?

A. No, it kept to the toe joint.

Q. How long did it last?

A. A week or a little more.

Q. Did the skin peel afterwards?

A. Yes, that's just it.

Q. Yet you have never had gout?

A. Not to my knowledge.

SPECIMEN SCHEDULES FOR "SPACE-FOR-TIME" REPORTS OF CASES.

SEVERAL of my readers having requested copies of the Space-for-time Schedules which I use for case-reporting, I am induced to issue specimens, thinking that they may perhaps prove useful to others. The expression "space-for-time" means that all periods of time are represented in the schedule by equal extents of space. Thus no time, be it a day, or month, or year, is left out, and the schedule conveys to the eye at a glance the whole duration of the case, with its different events in their proper places. I find this plan a great convenience, not only in chronological tables, but also in professional case-reporting. It saves the trouble of repeatedly making calculations in order to ascertain what stage has been reached. It is also invaluable when taking down a case *de novo* from the patient's statements, and avoids much risk of error. The patient is almost always able to fix some of his dates accurately, and these having been inserted in the schedule, others may be ascertained from them and his memory is assisted.

Blank schedules, in each of these forms, are always on my consulting table, and in all cases in which the history and the dates are important I "take the case" by their help. If the case is a short one, as, for instance, the primary and secondary stages of syphilis, one of these arranged in weeks or months is used. For such cases as hernia and abdominal obstruction, or other acute maladies, the one arranged in days is selected. The most generally useful, it being adopted for all chronic maladies, particularly for tertiary syphilis, is the one ruled for years. In some cases the early stages are spaced out on a ruled sheet by days or weeks, and the later ones, after it has passed into a chronic form.

[illegible] another by months or years. The Schedules are so extremely simple that I ought perhaps to apologise for thinking them worth publication. It is, however, their simplicity which is their great advantage. I have tried others with more complicated headings and much more letterpress, but have come back to these as being most helpful.

The space left at the top of the page is intended for the Case-heading. When the schedule has been filled up it should be pasted into a book and carefully indexed so that it can easily be found again.

Those engaged in family practice will find the short-period schedules very useful to have with the nurse in company with the Temperature Chart, and will in this way secure without much trouble valuable date-memoranda of their cases. It is obvious that the schedules are intended for nothing more than the very briefest notes, and are not designed to supersede, but only to assist, the writing out of case-narratives.

These forms can be struck off by any local printer very cheaply, but I may mention, for the convenience of any who may prefer to obtain them so, that my publishers, Messrs. West, Newman, & Co., are prepared to supply them and also books in which to place them.

PLATE CXXIV.

ENCHONDROMA OF THE PAROTID.

This portrait, taken from a photograph, represents the size and shape of an enormous parotid tumour. The patient was a man who was gardener to my late friend Mr. Edwards, of Keston. Mr. Edwards told me that he had known of the tumour for five and twenty years, and that it had been slowly growing without pain, and without much inconvenience except from its bulk. It had well illustrated the law of tumour-growth, *i. e.*, of population-increase, in that its rate of growth had increased in ratio with its size. The old man had always refused to have it removed. It finally, between the ages of 70 and 80, ulcerated, and brought about his death. I have seen one or two other parotid tumours almost as large, but none which had attained quite the dimensions which this portrait shows. In one which I saw under the care of the late Sir William Ferguson, its removal was followed by death. The lesson is obvious that these tumours ought to be excised whilst small.

PLATE CXXIV

PLATE CXXIV.

ENCHONDROMA OF THE [illegible]

[illegible] presents the [illegible]
[illegible] The patient was a [illegible]
[illegible] Edwards, of Keston. Mr.
[illegible] of the tumour for five and
[illegible] slowly [illegible] without pain
[illegible] from the back. It had well
[illegible] growth, [illegible] of popular [illegible].

West, Newman lith.

ARCHIVES OF SURGERY.

JULY, 1896.

SYMMETRICAL ACRO-SPHACELUS WITHOUT RAYNAUD'S PHENOMENA.

ALL who have studied in any detail the cases which have been grouped together under the name of Raynaud's malady will admit that the time has arrived when they ought to be classified. They are not all alike, nor do they all tend to the same results. It would be well, indeed, if we could cease to use the term Raynaud's Disease and speak rather of Raynaud's phenomena.* The latter expression is capable of having assigned to it a clear and definite meaning, the former is not. The phenomena in question are those of acroteric asphyxia, and are liable to variation at different times, being in many cases distinctly paroxysmal, and are to be explained by reference to the influence of the nervous system upon the blood vessels. They have their basis in physiology, and are in minor degrees exceedingly common. It is only when the reflex susceptibility in question is very much exaggerated, and when lesions of nutrition are threatened, that the conditions are to be ranked as morbid. Many of the cases which have been quoted as examples of "Raynaud," are, however, complicated or even primarily induced by organic disease of other parts;—of the skin (sclerodermia), the blood vessels or the heart. In all these, however, it is possible that the phenomena of paroxysmal or transitory asphyxia may be superadded.

* Raynaud's original Essay was translated by the New Sydenham Society, with comments and additional cases by Dr. Barlow.

Raynaud's phenomena consist, then, in paroxysmal contraction of the arteries of the extremities, leading in the first stage to dusky or livid congestion, and in the second to pallor, with the result in exceptionally severe cases of gangrene of the ends of the digits.

Although unfortunately Raynaud designated the affection which he described as "Symmetrical Gangrene of the Extremities," it is now certain that many cases in which the phenomena which he described are present are not attended by gangrene, and that some very definite forms of symmetrical gangrene are neither preceded nor followed by the paroxysmal phenomena referred to. I have in the present paper to deal with some very remarkable examples of the latter.

CASE I.—*A sudden illness attended by severe pain in the chest and symmetrical gangrene of all the digits, the nose, and the ears—Complete recovery of health.*

The man whose hand afforded the subject for the accompanying plate has often attended at my demonstrations, and I have already mentioned his case in a lecture published in the *Medical Week*, February, 1893, page 99. He has now been under my observation for three years, and I have repeatedly availed myself of opportunities for conference with my medical friends upon the facts of his case. The state of the circulation and of his heart has been carefully examined by several of our best authorities on such matters, with the uniform conclusion that no abnormality can be detected. No light has been thrown upon the nature of the disease which caused the gangrene. During the whole of the time that I have known him the man has appeared to be in perfect health, and has always had a pulse of good force, and been free of all tendency to coldness of his extremities. His history is that he had an acute and almost fatal illness, during which all his digits (both hands and feet) passed into gangrene, and his nose and ears suffered also. Before that illness he was quite well, and when he recovered from it he was again quite well. From first to last there have been none of the phenomena of paroxysmal asphyxia. The

West, Newman chromo.

question is what was the nature of his illness. Unfortunately I have not been able to obtain any detailed observations respecting it, and have been obliged to be content with the patient's own statements. My own hypothesis is that he passed through a severe inflammatory attack affecting the heart itself, and attended by extreme enfeeblement of the circulation at its centre. As to the probability of this the reader must judge for himself from the following statements.

The patient, an intelligent man of 37, a bookbinder by trade, tells us that during September, 1891, he was at his work in a warm shop, when he was suddenly seized by pain in one instep. The pain became so severe that he with difficulty walked home. During the next few days he managed to hobble about, lame from what he considered rheumatism in his feet. On consulting a surgeon he was ordered to bed, the suspicion being that he was about to have rheumatic fever. A severe illness of several months followed, during which it was feared that he would die. He was five months in bed. It was characterised, as he remembers, by extreme weakness and severe pain in the chest. In its early stage his fingers and toes, the end of his nose and edges of his ears became dusky and livid, and rapidly passed into gangrene. In the course of six weeks from the beginning the fingers were quite black. From his nose and ears fragments of dry gangrenous skin were detached, and from all his digits the terminal portions were also lost. The length of the portions which subsequently slowly separated was, in the case of the fingers, about an inch and a half, comprising the last phalanx and the greater part of the middle one. In the case of the toes the length of the fragment was much less. In all, although the mummifying process was soon complete, the detachment was a matter of months, in the case of one digit being more than a year. I saw the man for the first time about eight months after his illness, and at that time he was in good health and had a pulse of fair power. His nose and ears were then healed, and so also were most of the stumps of his digits. In all instances the healed parts were quite sound, and showed no evidences of feeble circulation. The symmetry in the parts attacked had been most accurate.

It remains to add to the preceding account that the man was not of intemperate habits, that he had never had syphilis, and that he had not been exposed to malaria.

A case which I find well recorded by Dr. Allman Powell in the *British Medical Journal* for 1886 was in many respects so much like my own that it may be well to narrate it before offering any comments. It occurred in the practice of Mr. Hyde in the General Infirmary, Worcester.

CASE II.—*Symmetrical Acro-sphacelus occurring as a single attack in an adult man previously in good health, and followed, after separation of the sloughs, by complete recovery.*

The following is an abridged narrative of this case:—A policeman, aged 48, who had enjoyed excellent health, was attacked on the 12th of May, 1884, by sore throat. It was followed by loss of voice, and diphtheria was diagnosed. The gangrene of extremities set in three weeks later, during convalescence. He noticed one morning that the end of his nose was blue and swollen. A day or two afterwards his ears were affected, and the pulps of all four fingers of his right hand. In the following week the little and ring fingers of the other hand were attacked. Throughout, both thumbs and the index and middle fingers of the left hand remained free.

It was not till July 14th that the patient went to the hospital, and came for the first time under Dr. Allman's observation. The gangrene had then been in progress more than a month, and lines of demarcation were forming. His nose was cold and livid. The helices of both ears were blue, and there was a small dry slough on one of them. The distal thirds of the affected fingers were in a condition of mummified gangrene, and so also the ends of the second and third toes of the right foot. On both hands the digits which had not gone into gangrene were "cold, glossy, devoid of hairs, and of a dusky red colour at their tips." The patient at this stage had fairly regained his health; he had no paralysis, no signs of disease in the chest, nor were any of his arteries occluded.

PLATE CL.

SYMMETRICAL GANGRENE OF DIGITS WITH [illegible] PHENOMENA.

I have taken a [illegible] of
had a coloured [illegible]
way the [illegible]
[illegible]
[illegible]
[illegible]
[illegible]
[illegible]

West, Newman chromo.

His temperatures were normal, but his urine contained a little albumen. In describing the early stages of the gangrene the man said that at first the whole of the fingers became almost black, but by degrees the discoloration receded until only the terminal thirds were involved. There was much pain at this stage, and he became weak and thin.

The sequel of this case, like my own, was perfect recovery, with sound stumps and no tendency to paroxysmal recurrences. The sphacelated digits were removed about three months after the commencement of the malady, and the wounds left healed slowly. In the following year (August, 1885) Dr. Powell saw the patient on duty in an assize court in excellent health.

A woodcut which is given in illustration of this case shows precisely the same conditions as regards the length of digit involved, &c., as in my own case.

Comments.

These two cases appear to agree in the following points: In both the patient was a robust man who had never had any serious illness before. In neither was there any former history of rheumatism, gout, or syphilis. In neither had there been any peculiarity of the circulation observed previously. In both the stumps healed soundly, and the restoration of the circulation appeared to be complete. No occlusion of the distal arteries was ever proved in either case, and in both it is certain that in the later stages they are quite pervious. No disease of the heart was proved in either. In both the onset of the attack was sudden. In both not alone the hands, but the toes, the ears, and the nose were involved.

It is much to be regretted that in neither case have we any accurate information as to the state of the pulse or of the heart during the onset of the attack. Both patients had a serious illness at the time the gangrene commenced. In one this illness was diagnosed as diphtheria, whilst in the other rheumatic fever was suspected.

In both cases the evidence makes it probable that a very definite condition of asphyxia or venous turgescence was

present in the early stage, and persisted until the gangrene was complete.

Against the belief that there was any mechanical occlusion of the affected vessels we have the fact that the sphacelus was symmetrical, and affected the nose and ears as well as the digits. It is impossible to believe in plugging of vessels as a cause of gangrene of the nose-tip. At one time in Dr. Powell's case, it is recorded that the tongue was swollen and dusky, and the mouth generally painful.

In my own case, which was much the more severe of the two, there was no suspicion of diphtheria. We are obliged in these days of new and powerful drugs to keep in mind as a possibility that the circulation may have been depressed by inadvertent persistence in some supposed remedy. It is certain that neither patient had taken ergot, nor indeed were the symptoms like those of ergotism.

We are probably on safe ground in believing either that there was present a severe affection of the central organ, or that in some way or other, either from without or from within, a poison had gained access to the blood, which had the effect of producing temporarily persistent closure of the peripheral arteries. The nervous system may have taken its share in the process, but not probably a primary one.

In the sixteenth volume of the Pathological Society's Transactions there is a case recorded which in some respects resembles the two which I have just narrated. In it, however, the sphacelus was not nearly so extensive, and there was also reason to suspect that the arteries of the limbs were obliterated.

My next case differs from the two given above in that the patient was senile, and the subject of advanced disease of the arteries. It may be fairly claimed as essentially one of senile gangrene. The latter affection, however, but rarely attacks the upper extremities, and still more rarely makes any approach to bilateral symmetry. The latter phenomenon can hardly find its explanation elsewhere than in some central cause. It could scarcely be a mere accidental coincidence that the two hands were attacked together, and that in each almost all the digits suffered alike.

PLATE CXLIX.

SYMMETRICAL GANGRENE OF DIGITS IN CONNEXION WITH CALCAREOUS ARTERIES.

This portrait shows the condition of the hand in the Yarmouth patient. It has been lithographed from a sketch which was kindly made for me by Dr. Ragg, then the Resident Medical Officer of the Hospital. It exhibits the emaciated, withered, and discoloured hands of a very old man. In comparing it with the other portraits now given, much allowance must be made not only for these facts, but for difference in artistic treatment. The deep shading must not mislead the observer, for it is only on the quite black ends of the digits that there was any gangrene. The rest of the hand was simply atrophic and pigmented. The extent of the gangrene is clearly shown, as also the fact that it was abruptly limited and of the mummifying type.

CASE III.—*Symmetrical Acro-sphacelus in association with senility—Extensive and advanced calcification of the arteries—Nose and ears exempt—Fingers more severely affected than toes—Advance of gangrene on the feet—Patches on knees and hips—Death.*

During a visit to Great Yarmouth in December, 1894, I was shown in the Infirmary a very interesting case of symmetrical acro-sphacelus in connection with senility. All the digits of both upper and lower extremities were affected, and with almost exact symmetry. Much longer portions of the fingers were involved in gangrene than of the toes. Of several of the fingers an inch and a half of length was black and mummified, looking as if the man had finger-stalls of black kid on. Of most of his toes only just the tip was involved in gangrene. The gangrene was not spreading in any one place after the manner so usual in the senile form. It was everywhere dry, and exactly like that in the other cases.

The patient in this case was an old man of nearly eighty, very thin, and with a bushy white beard. I took him for a Jew, but this was denied. His radial arteries were quite rigid, and ringed like ipecacuanha root. Neither Dr. Blake, who was present, nor myself could detect any pulsation in them. The old man could give us no suggestion as to the cause of the outbreak of the gangrene. He said that he had been well cared for, and not exposed to cold, and that the gangrene began in October when the weather was warm. He had not suffered from any depressing illness, and was indeed in his usual health when the extremities of all his digits began to ache and become numb.

I am indebted to Dr. P. M. Ragg, the resident medical officer of the Infirmary, for notes of this case, and also for an excellent drawing from which Plate No. CXLIX. has been produced. The patient was under the care of Mr. Lettis.

I have been informed that the man remained in the Hospital for three months after I saw him. The gangrene spread on both feet till it involved nearly the whole of them. There were also developed patches of gangrene on the knees

and the hips. The patient was ultimately removed to the workhouse, where he died in the beginning of April.

It is difficult to conceive of any cause which should suddenly initiate symmetrical gangrene, excepting some loss of power in the heart. No doubt the calcareous condition of the arteries predisposed to it, and rendered the circulation in the extremities difficult of maintenance. If in addition to this some sudden obstructive disease occurred in the heart, one can easily understand that the peripheral parts might pass into gangrene, and that, if the circulation were restored, the gangrene might remain isolated. Thus the case should be viewed, I think, as being in part from calcareous arteries, and in part of cardiac origin. It is to be added that the man had been under Dr. Blake's care two years before with sphacelus of the small part of one toe, from which he had recovered. It did not appear that his nose or ears had been in the least threatened, an exemption probably due to the comparative absence of calcareous changes in the arteries of the head.

APPENDED CASES CITED BY RAYNAUD.

The following cases which I abstract from Raynaud's Essays appear to belong to the group under consideration. They are, that is to say, examples of gangrene of many digits or of entire limbs with more or less close symmetry occurring to persons who had not previously shown any liability to disturbances of circulation, and who afterwards, with one exception, recovered completely. In the excepted case death occurred during an illness in which not only the digits but both feet and both hands were, as wholes, involved in gangrene.

CASE IV.—Raynaud's Essay, Case xv.—A married lady of 27, who had had excellent health, but had been very prone to chilblains. Her hands were habitually red. After her second confinement she had a severe illness with diarrhœa and great weakness. Three months after this confinement, in winter, her finger ends became red and painful, and in the course of a month were gangrenous. The toes assumed a similar but less pronounced condition. The pain was extreme. The end of the nose

was threatened. The radial pulse remained good. The patient was not seriously ill. Gangrene of skin and nails of most of the fingers and toes followed. The process was complete in about eight months, and the patient regained good health.

In this case premonitory indications of feeble circulation had not been wholly absent, and the history after recovery does not extend over a long period. The next case is, however, more definite, and I may add that it appears to have much resembled that of my own patient. *See* Case I.

CASE V.—Raynaud's Essay, Case XVI.—The subject was a robust man of 34. He had lived freely and had had syphilis. After some exposure and possibly under the influence of malaria, his fingers and toes became numb and painful. He had severe pains in the joints, and it was thought to be rheumatism. He was of deathly pallor, and had brain symptoms, with extreme prostration. Portal saw him and recognised dry gangrene. The ends of the fingers and of the toes from the first to the second phalanx became as black as coal and as hard as horn. The digits were amputated with forceps and saw. The stumps of the patient were soon perfectly cured.

CASE VI.—Dr. Bernard Henry's case (Raynaud, No. XVII.).—A dissipated woman of 46, who had had syphilis. Acro-sphacelus of all four extremities. Line of demarcation through fore-arms and lower thirds of legs. Nose and ears threatened, as also the fronts of patellæ. Death from exhaustion in third month of attack. Main arteries found closed at line of demarcation. The heart showed only a tendency to fatty degeneration. The details in this instance are defective, and the same remark applies to the two others which I have next to mention.

CASE VII.—Molin's case (Raynaud, XXIV.).—A young woman lost both feet and many of her fingers by gangrene, and then recovered her health and even became stout.

CASE VIII.—Raynaud XXV.—A girl of 7, after an ordinary fever, had mummifying gangrene of both hands, and lost them by separation below the elbows. She recovered, and was well when seen by the French Academy at the age of 21.

CASE IX.—Case by Mr. Gintrac, quoted by Raynaud. A married lady of 48, in whom menstruation was ceasing, experienced suddenly pain and coldness in the fingers of both hands. Her toes were not affected, nor were her thumbs, excepting that she had pricking pains in them. Detachment of the ends of her fingers by dry odourless gangrene proceeded very slowly; when it was completed she regained health, and she was quite well three years later. No circulatory causes could be discovered.

NOTES ON LEPROSY, WITH ESPECIAL REFERENCE TO ITS DIETETIC CAUSE.

I HAVE collected, partly by reading and partly by conversation with those who have visited leprosy districts, a great deal of fragmentary information respecting the conditions under which it prevails. In now publishing some selected portions, it is hoped that my readers will not attribute to mere idleness the circumstance that they appear in a somewhat unconnected form. For myself I may confess that I often find fragments of this kind both more readable and more impressive than an elaborate essay.

Certain objects have been kept clearly in view in the selection of these items of evidence. They may be stated somewhat as follows:—

1. To show that fish is an article of food largely consumed in almost all leprosy districts.

2. To show that the disease (the same in all countries) has probably been indigenous in many, and that it prevails under conditions which do not support the belief that it has spread by contagion.

3. To show that its extinction in countries where it has ceased to prevail has not been accomplished by measures of segregation, but has ensued under the influence of advancing civilisation—in other words, of change of food.

Illusory Instances of Contagion.

The "Grandes Chroniques de France," 1505, relate how a noble lady, Yolande de Vallières, of loose life, became a leper, and was discarded by relatives and abandoned by lovers. A clerk who had been a scorned adorer now received her into his house, tended her most lovingly till her death,

in memory of her beauty, and disdained not to kiss her leprous lips. "Aussy est il mort de ceste mesme maladie abominable. Cecy advint près Fontanu bellant en Gastinois." On this story Swinburne's "The Leper" is founded.

Pathetic stories such as the above and that of Father Damien are taken by many as conclusive instances of contagion. They are no such thing. In each instance the person supposed to receive the disease by contagion was living in a place where leprosy was endemic. Obviously he was exposed to other influences, and if food be one of the causes of the disease, he might have so acquired it. Examples of contagion, to be trustworthy, must be found under conditions which exclude other possible causes.

Leprosy at Lincoln and Stamford.

Some interesting particulars respecting the Leprosy House at Lincoln were collected by Dr. Cookson, and read by him in 1841 before the Topographical Society. The place where the leprosy house stood is now known as the "Malandry Closes," and stands on the Lincoln and Sleaford turnpike, just outside Little Bargate, the ancient south entrance to the city. The term "Malandry" Dr. Cookson supposes to be a corruption of the Norman-French "Maladerie," a name for a lazar-house. From Dr. Cookson's paper I extract the following items.

The distinction between a "Bede-house" and a "Maladerie" was, in some places, in the Middle Ages, well observed, the former being for sick and infirm, and the latter for lepers. In the case of Brown's Hospital at Stamford, the regulations, as compiled in Henry VII.'s reign, ordained that no leper man or woman be admitted or put into the said almshouse, lest he should affect his whole or sound fellows, and provoke them to "loathsomeness or ogglesomeness, and he shall be removed and brought to some other place where he shall be received." The Deans of Stamford and Allhallowes were entrusted with the execution of this statute in perpetuity. As recently as the year 1678, John, Bishop of Lincoln, in a revival of the statutes, repeated this provision against the admission of lepers into the almshouse.

Leper Home at New Romney.

At New Romney (see "Archeologia Cantiana," vol. xiii. page 247) a leper hospital was founded between 1184 and 1190. It still contained lepers in 1255, when Robert, a leper, the husband of a certain woman named Amicia, was an inmate. In 1363 it was ruinous, and was founded anew on a fresh basis as a perpetual chantry (presumably because there were no more lepers).

Erroneous Statistical Statements.

In the Cantor Lectures on food for 1868, the author makes the following statement (see page 46): "About 4,200 tons of fish; over 4,000 sheep; nearly 700 oxen; about 90 calves; 4,000 pigs (including bacon and hams), &c. . . . are daily brought into London." In reading this enumeration I was at once struck by the fact that for every single sheep a ton of fish was furnished, yet it seemed impossible to think otherwise than that Londoners eat more mutton than fish, and it might take the carcases of ten sheep to make a ton. It also occurred to me that as almost all the London fish is distributed from Billingsgate, and in carts, it would require more than four thousand carts, each carrying a ton, to effect the daily distribution, and that such a number would entirely block the streets near the market. I told my dilemma to the author (long since deceased) of the Lectures, and he assured me in the most positive terms that the statement was correct, adding that, as Medical Officer for the City of London, he had all the returns through his hands and that there could be no error. I avowed my incredulity. A fortnight later I met my friend again, and he then told me that he had looked into the matter and found that the 4,200 tons ought to have been 800. He could give no explanation as to how the error had occurred. Now the Cantor Lectures were delivered before the Society for the Encouragement of Arts Manufactures and Commerce, and their author was Medical Officer of Health and Food Analyst to the City of London. Here, if anywhere, one might have hoped to find statistical statements which might be trusted. Yet the book abounds with statistics which it is, for the most part, impossible to confute,

but many of which, for my part, it is impossible to trust. Had this statement as to the quantity of fish consumed in London been quoted without the context, I should have had but little clue to its absurdity; and had it fallen into the hands of a foreign writer, I have little doubt that it would have been held to be incontrovertible. It might have been quoted from book to book, and we might have been told that a strong argument against the fish-eating theory of Leprosy was that the Londoners eat ten times more fish than mutton and yet remain free.

Fish-Eating and Fast-Days.

Under the ritual of the older Church we were accustomed during the Middle Ages to fast forty days at Lent (except Sundays), the Wednesdays and Fridays in Advent, Ember days, four times a year (making altogether twelve days) and on five vigils. Abstinence from flesh meat was also enforced on all Fridays, excepting when Christmas Day fell on a Friday.

This would give nearly a hundred days in which fish only could be taken. It must be supposed that the artificial demand thus created would have the effect of widely diffusing the supply of fish, especially salted, and of thus making it obtainable at all times and in places to which otherwise it would scarcely have found its way. When Lent was over it might often happen that the fishmongers had some overplus which they would be anxious to get rid of. Neither they nor the fish-catchers would be willing that their trade should wholly lapse in the intervals of the greater fasts. We may therefore, I think, fairly assume that the ordinary consumption of fish, quite apart from that taken on fast days, would be far greater than it is at present.

Roman Catholicism and Leprosy.

I can feel little doubt that Roman Catholicism, with its frequent fasts, was the innocent cause of the increase of leprosy in the Middle Ages. These ages were emphatically those in which Catholicism had most influence, and those countries appear to have suffered most which were most

devout. We have no record of corresponding prevalence amongst the Mahommedans. Undoubtedly it was present to some extent in most places before the introduction of the Catholic Ritual, but the remarkable increase occurred simultaneously with the spread of the latter and precisely where it went. More especially, in all probability, does the compulsory consumption of salted fish explain the spread of the disease in mid-Europe and other places at a distance from the supply of fresh fish. To this day leprosy lingers in some Catholic communities when adjacent countries are free (Spain, Portugal, parts of France, Crete, Sicily, &c.). The salt cod of Bergen is sent to Portugal, &c., for the fasts, and where it goes there some leprosy is still to be found. The same is true of some of the West Indian Islands.

On the collection of Statements of Popular Beliefs as to the cause of Leprosy.

Although popular beliefs are often entirely erroneous, yet it may be desirable to collect them and allow them such weight as they may seem to merit.

I have elsewhere referred to the fact that the Somalee tribes are supposed to have been so strongly convinced that fish was the cause, that they resolved to abstain entirely. The following extracts record opinions formed nearer home :—

"The more prevailing notion is that the leprosy was generated by the eating of salmon too frequently and at unseasonable times. That our forefathers thought so is evident from covenants which I have seen in this county and in Devon, stipulating that no apprentices or servants shall be obliged to dine on salmon more than once or twice in a week. And we are told that in consequence of a due abstinence from salmon, lazar houses became no longer necessary. In the same manner this disease is said to have prevailed in Ireland till the English laid the Irish under restrictions in the use of salmon" (Polwhele's "History of Cornwall," vol. ii., p. 88).

By what means the tyrannous English succeeded in depriving the inhabitants of the Sister Isle of their salmon, I have no idea. Possibly some of my readers can help me.

Carew gives us the following statements :—

"Lazar houses, the devotion of certain Cornish gentlemen's ancestors, erected at Minhurst by Liskerd, St. Thomas by Launceston, and St. Lawrence by Bodmyn; of which this last is well endowed and governed. Concerning the other I have little to say, unlesse I should eccho some of their complaints, that they are defrauded of their right. The much eating of fish, especially newly taken, and therein principally of the livers, is reckoned a great breeder of those contagious humours which turn into leprosie; but whence soever the cause proceedeth, dayly events minister often pitiful spectacles to the Cornish mens' eyes of people visited with this affliction, some being authours of their own calamity by the fore-mentioned diet; and some others succeeding therein to an hereditarius morbus of their ancestors; whom we will leave to the poorest comfort in miserie, a helplesse pittie."

Popular Opinion in China.

To Mr. Cahill, the intelligent first officer of the *Chimborazo*, who had sailed often on the coast of Norway, I put the question—"Have you seen much of leprosy?" "Not much in Norway, but plenty in China," was his reply, adding: "You mean, I suppose, 'Fish leprosy'?" "Why do you call it Fish leprosy?" "Oh because it comes from eating bad fish." "Is that the theory in China?" "Yes, it is what I have always been told." "You are in advance of your age." "Well, every one says so on the China coast. You know they eat such horrible messes of fish stuff such as no European would touch."

Segregation imperfect.

The Leper Asylum of Seychelles on the 3rd of August, 1851, contained 32 lepers, and 18 persons who suffered from other diseases.

In the Dhurunsalla, a private refuge at Bombay, the lepers and the blind were placed together.

Such facts as these seem to indicate that really there was but little fear of contagion in those who had the manage-

ment of these establishments. They are also most instructive as showing how leper asylums have always been managed, and how absolutely inefficient they must have been to exterminate the disease if it were really contagious. Our English leper hospitals, during the decline of leprosy in Britain, were allowed to admit other diseases, and it cannot be doubted that chronic skin diseases of all kinds were taken into them.

France in the Thirteenth Century.

Louis VIII. of France reigned from 1223 to 1226. He left legacies for the relief of lepers—some accounts say to 2,000 *leper houses*, and some to 2,000 *leprous persons*. The difference is important if we desire to estimate the prevalence of leprosy in France at that period.

Fallacies of Statistical and Partisan Statements.

Dr. Beaven Rake, in his last report of the Trinidad Leper Asylum, gives us some interesting information respecting the value of statistical statements. After directing attention to various inaccuracies in Archdeacon Wright's statements, he mentions that, whilst one affirmative reply by a Trinidad surgeon to the inquiry as to transmissibility by vaccination is published, the fact is omitted that thirty or more replied in the opposite sense.

Dr. Rake tells us that in Père Etienne's book, "La Lepra est Contagieuse," it is stated that in 1805 there were three lepers in Trinidad; in 1813, 73; in 1817, 77; and that in 1878 the number had increased (presumably from contagion) to 860. Archdeacon Wright has been more moderate as to the present number, and states it at 480. Dr. Rake can find no authority for any of these statements, and has failed to ascertain that any Leper Census was taken prior to 1889. He calculates the present number at 348.

The destruction of Lepers not efficient to prevent its spread.

The cruel practice of burying lepers alive, as formerly practised throughout India, was a far more efficient means of preventing contagion than any plan of isolation which

ever obtained in Europe. It never, however, succeeded. In spite of their removal in this way, the supply of fresh lepers was perennial. It is even a matter of doubt amongst those best informed whether there has been any increase since this practice was forbidden.

The Molokai Leper Establishment (Sandwich Islands).

The report of 1888 (Mr. Meyer) of this establishment showed that there were in the leper settlement 749 lepers. Out of these there were two Germans, one Pole, one American, one Belgian, one Russian, 23 Chinese, one Raratonga native, one South Sea Islander. Thus, more than 700 were Hawaiians or half-castes. Living in the same settlement with the lepers were 144 of their friends and 49 natives of the place. So much for segregation.

Immigration into Sandwich Islands—Mortality.

During two years preceding the report of 1888 there were landed at Honolulu, for purposes of quarantine, 4,800 persons, of whom 3,294 were Chinese, 1,472 Japanese, and 34 other nationalities. The death rate for two years was no less than 45 per 1,000; that of the Hawaiians themselves far exceeding any other nationality.

The Chinese at Singapore.

Writing of Singapore, Mr. Caine says, "The supply of food of all sorts is, as usual, in the hands of the industrious Chinese, who catch the fish," &c., &c. (page 250).

It is well known that leprosy goes wherever Chinamen go; and wherever Chinamen go, there they become the cooks and food-providers.

Hawaiian Islands and the Chinese.

"The natives derive their maintenance chiefly from pork and fish, both fresh and dried."

The gross population is about 58,000, of whom about 6,000 are Chinese, or more than one in ten. Every tenth person is a Chinese.

"Whilst the Hawaiians are decreasing, the Chinese are coming in large numbers, and threaten in time to take their place. . . . The Chinese are the most numerous of foreigners: they are acquiring property rapidly, and some of them are wealthy" (E. N. Johnson, in "Encyl. Brit.").

On the possibility of Effectual Segregation.

Dr. Beaven Rake, in speaking of compulsory segregation, and of the control of immoral lepers, admits that it would be necessary "to convert the asylum into a jail, and build a high wall all around it."

Against Hereditary Transmission and Contagion.

As regards the risk of hereditary transmission of leprosy, we may note that Father Damien permitted the marriage of lepers in Molokai. Dr. Fitch, in his appendix to a report on Leprosy in Hawaii, in 1886 (p. 31), gives some interesting facts as showing that there is but little risk of the increase of leprosy by the marriage of lepers. He at the same time incidentally supports the belief that there is little or none of contagion. He states that, between 1866 and 1884, 2,864 persons had been confined to the leper settlement at Kalawayo; and that in the latter year there were only 26 children alive who had been born of lepers in the settlement. Of these 26, although in addition to their being offspring of parents who were one or both of them lepers they had also lived in the leper district all their lives, only two showed signs of the disease.

Supposed Increase in India.

The supposed increase of leprosy in India is probably only apparent, and is to be explained in part by the circumstance that the burying alive of lepers is no longer permitted. It would be as reasonable to allege a real increase in the number of widows and of female children since the abolition of suttee and of infanticide. To some extent, however, the increase of leprosy may be real, and may be in connection with increase in the production of salted

fish. This increase has, I believe, been especially great along the Madras Coast in the hands of Europeans. Surgeon-Major Pringle, in a paper in Reports of the Epidemiological Society, has drawn attention to the disuse of the custom of burying alive as an explanation of recent increase. It is true that it would not explain a quite recent increase, since the British protection of leper lives has now been in force for many years; and the life of an individual leper is not usually more than twenty. It is well known, however, that in English districts adjacent to native territory it has often caused the immigration of lepers from the latter into the former. To those—of whom Surgeon-Major Pringle is one—who believe leprosy to be contagious, the preservation of lepers alive is a ready means of explaining increase; it obviously multiplies the foci for contagion. Such a supposition would, however, account for far more than has really taken place. If leprosy be a disease which spreads solely or chiefly by contagion, surely it ought to have enormously increased since the promulgation of the British edict of mercy. Burying alive is by very far more likely to be efficient than any plan of isolation or segregation, and its sudden interdiction ought to have led to a rapid spreading of the disease, and that, too, in rapidly increasing ratio. Yet the most recent statistics, those collected by the Leprosy Commission, tend to show diminution rather than increase.

New Hebrides.

In the course of official duty, I have had to read a very interesting report on the New Hebrides by Staff-Surgeon Cross, of H.M.S. *Diamond.* From it I borrow the following items as to food:—"Although plentiful on the coast and among the reefs, fish are, after all, only obtained in small quantities, this generally of the mullet species." The natives, according to Mr. Cross, are very particular in their cookery. No mention is made of salted or of decomposed fish. The explanation of the small quantity of fish taken is that they do not know how to catch it; and secondly, that other food is abundant, and they are a lazy

race. The principal foods are yam, taro, and cocoa-nuts. It may be that in many aboriginal tribes the inability to catch much fish may much diminish its use, and we must always keep this in mind in attempting to estimate the probable consumption of fish by uncivilised races. As a rule, no doubt, savages, being near the sea, consume fish and mollusks largely, but there may be exceptions. Precisely similar statements apply to the prevalence of leprosy.

The Desert Rajpoots willing to forego food-prejudice.

Sir Bartle Frere thinks that "some of the very general and inveterate Hindoo restrictions" existing in modern India are "of comparatively recent introduction." He writes, "Thus the Desert Rajpoots of the Ioda tribe till lately made little objection when in the field to eat from the same dish and of the same food as men of other races."

Hindoo prejudice to Birds as Food.

Writing of the Hindoos of the Thurr (Rajpootana), Sir Bartle Frere states: "Thus the Hindoo prejudice against anything feathered is shown in a kind of proscription of fowls, which are bred only by Bheels and other low-caste races. One Christmastide we were anxious to have a plum-pudding, and inquired for eggs; but it appeared that there were no eggs in the country, and one local official, after careful inquiry, reported that we were fifty miles by estimation from the nearest egg, which it was supposed might be found in the possession of a tribe of Bheels at that distance from us." *

Leprosy and Syphilis. Do the Brahmins eat Fish?

A gentleman who had lived long in India told me that at a leper hospital, with which he was familiar, it was estimated that between 60 and 70 per cent. of the lepers were the subjects of syphilis also.

I spoke with him about the fish-hypothesis. He said that every one in that district ate fish. I asked him whether

* Journal of the Geographical Society, 1870, p. 206.

there were not those whose religion forbade it? He said, "Yes, the high-class Brahmins professed to eat no flesh; but it was a common joke that a Brahmin would take fish home because his cat was fond of it."

Do the Hindoos really abstain from Fish?

A missionary lady, Mrs. Winckworth Scott, who had resided at Delhi, and had had good opportunities for observing the native habits, assured me that it was very difficult to ascertain the truth as to what they would not eat. She added that one of her bearers, who was strict in his religion, would yet at times eat fish in a condition in which she could not touch it. "They have funny notions."

South Africa.

A railway engineer from South Africa told me that the Zulus will not eat fish; but the Zanzibar men will eat fish but not meat. Fish salted is, he said, largely imported for the Zanzibar boys.

The Singapore Fish Market.

The Singapore fish market is thus described by Mr. Caine: "After ducks come fish, which are caught in great quantities in the bays and straits of the archipelago of islands of which Singapore is the centre, and which are brought to the market fresh, or rather alive, twice a day. Cuttle-fish are in great demand; crabs shaped like long-tailed fans. . . . Prawns, six inches long, are prized for curry, and the variety of fish of all sizes ranged from a twelve-foot shark to the tiniest transparent whitebait. The shark appeared the staple food of the poor in fish diet, but the bonita, a fish that looked like a two-foot herring, small sword-fishes, gar-fish, a creature that seemed all fins and head. . . . One whole side of the market is given up to dried and salt fish which made one thirsty even to look at. Another avenue is devoted to 'chow chow,' or cooked food of all sorts, where groups of Chinese and Malays were squatted about enjoying fearsome-looking dainties of various kinds and flavours."

Fish-eating in Ceylon.

Dr. Anthonisz, who had lived in Ceylon many years, gave me much interesting information about leprosy there. All curries in Ceylon are made with fish. It is a half-salted, half-dried fish, which is a large article of commerce between the Maldive Islands and Ceylon. It is grated into every vegetable curry to give flavour, and is sometimes eaten without being cooked. The fish which goes to the interior is often decomposed. The name of the Maldive dried fish is Combalmas, or in Cingalese Umbala-cada. It is hard like horn, and is very good as a condiment.

Through the kindness of a medical friend, I have a specimen of this dried fish in the Clinical Museum. It is as hard as wood.

Leprosy in Persia.

One of the strong arguments used by the opponents of the fish theory as regards leprosy is that the disease prevails extensively in the highlands of Persia, where, it is said, fish cannot be obtained. I have the authority of Captain Vaughan, who has travelled much in Persia in connection with geographical surveys, for the following statements. Captain Vaughan, although not a medical man, has lived in India, and is familiar with the appearances of leprosy. He tells me that in Persian villages the peasant will insist on regarding all Europeans as doctors, and that he himself had many sick persons brought to him. So far, however, as his experience has gone, there might be no leprosy whatever in Persia. Next, as regards the possibility of obtaining fish, he states that in all parts dried salt fish is in common use, and that, even where there are rivers from which good fresh fish may be obtained, the natives prefer the salted. He added also that all along the shores of the Persian Gulf great quantities of shrimps are taken, which are preserved and sent inland. Thus, admitting what I think is very probably the case, that a small number of lepers are to be found in Persia, it is clear that such a fact is by no means conclusive against the suggestion which would connect the disease with the consumption of salt fish.

Leprosy in Arabia and Persia—Facts as to Food Contagion, &c.

Mr. T. I. W——, an intelligent surgeon who had travelled much, gave me some interesting information on various points (November 14, 1890). He said that he thought the disease must be contagious, and mentioned that he had known a man who married a leprous woman become a leper himself. I urged that this proved nothing, since the man had lived under similar conditions to those under which the woman had acquired the disease, eaten the same fish, &c. I urged also that it never spread by contagion in England. Mr. W—— admitted the force of these arguments, and added that he also had witnessed many negative facts. He said that he had been often puzzled, on the supposition of contagion, how to account for the fact that it does not spread more freely. "The Chinese, for instance, live in a state of crowding to which the most densely populated parts of London afford no sort of parallel; yet lepers live amongst others, and they do not appear to catch the disease." I told him that the same fact had often seemed to me conclusive against contagion. In those parts, and they are many, where no precautions are taken, all the community would become lepers if the disease could spread by contagion with an ease approaching, for instance, that of syphilis. Mr. W—— told me that in no country did the facts more favour the fish theory than in Arabia. There, he said, leprosy was almost unknown inland and abundant in coast towns (Hydramott). In the latter the people were obliged to eat fish largely. In Persia, he said, salt fish was a luxury, expensive, and not an ordinary article of food. In Turkestan and on the eastern frontier of Persia (he spoke of Khorasan) leprosy was, he believed, almost unknown, whereas it became abundant on the shores of the Persian Gulf. In Persia generally fish food is regarded with prejudice as unwholesome.

Fish Diet in Leper Houses.

The lepers in the asylums at Molokai (Sandwich Islands) receive per week seven pounds of fresh fish and three pounds

of salted salmon. By this we may judge as to the diet of the inhabitants generally. It is important to note that, although the use of fish for food has for ages been common in the Sandwich Islands, the employment of it in the salted form is novel, and that the establishment of salting-houses coincided with the great increase of leprosy.

Raw Fish eaten in Japan.

Sir Edwin Arnold records, in his "Seas and Lands," that, at a banquet given by a rich Japanese merchant, the *pièce de resistance* was composed of raw fish of several descriptions.

Samoa and Fiji.

Leprosy has been probably known from immemorial times in the Samoan and Fiji Islands. The Rev. Mr. Waterhouse records that the natives of the latter have a tradition that the Kava root grew originally on the grave of a Tongan leper. It is said that it is not now known in the Samoan group.

Polynesia.

"In some of the atolls where the people have little good vegetable food and eat a great quantity of fish, much of it often in a state unfit for food, skin diseases are even more common than in the mountainous islands. There are repeated cases of leprosy in the Gilbert Islands, and that disease is well known to be one of the scourges of the Hawaiian archipelago" ("Cyclop. Brit.," vol. xix. p. 422).

Recent Extinction of the Disease in New Zealand.

Mr. T. W. Bell, in some recent medical notes on New Zealand, writes: "A form of leprosy used to exist among the Maoris. It caused little pain, but the fingers and toes and even the limbs dropped off (Colenso and Thompson). *I have never seen a case.*"

Bahamas.

The Rev. Mr. ——, who had lived in the Bahamas, told me that leprosy was common there, and that consumption of fish in all forms was very large. They would, he said, eat "conchs" raw.

Lake Tchad.

What are the facts as regards leprosy in the neighbourhood of Lake Tchad? It is said to prevail there, but I do not know who has recorded the evidence. If it does, it is a very strong fact in favour of the fish hypothesis, for the lake is a little sea in a low district, and abounds in fish.

An Error Avoided.

The late Dr. Beaven Rake, with praiseworthy zeal, examined, in the search for bacilli, the earth over the graves where lepers had been buried. His investigations might easily have led a less cautious observer into grave error. He found bacilli on the surface of eight lepers' graves, with the corroborative fact that the earth in which they occurred was stained deeply. Over the grave of a leper on whose body no post-mortem had been made, and in whom presumably the bacilli were confined, no traces of them were found in the earth. Thus far, then, the evidence might seem to be strong. It was found, however, that similar bacilli were present in the earth over the grave of a patient who had been admitted by mistake into the asylum, and who was the subject of phthisis, and not leprosy; and, finally, in examining the earth of his own garden, a mile away from the leper-house, he found the same baccilli, and the earth took the same stain. It was thus tolerably clear that the bacillus in question had no connection whatever with leprosy.

South America.

In a history of America, published in 1783, I find leprosy mentioned as endemial in New Carthagena (Granada), and "gaining ground every day." A hospital was erected for it, and the patients admitted were allowed to intermarry. It is still common there.

"The reason why the south-western part of the country is that most frequently attacked seems to me without doubt to be that it is the district into which the disease was imported, and has been able to take firm root among the poor population, consisting of fisher-folks."

NOTES ON MALADIES WHICH ARE LIABLE TO RECUR PERIODICALLY.

(*Concluded from p.* 169.)

The reader will find in a report on "Catarrhal Eruptions" (Vol. V., p. 203), a number of cases given which illustrate recurring maladies. Catarrhal affections, indeed, divide with herpetic ones the claim of predominance in this group. Probably a large majority of the maladies which are recurrent stand in some relation with catarrh, and between herpes and catarrhal disorders there is a well-recognised connection. In the paper referred to eight cases are recorded, most of them examples of vesicating or erythematous eruptions on the hands, feet, and occasionally on the face, and all more or less allied to cheiro-pompholyx. To one of these I will now recur, as I have just had an opportunity of again seeing its subject during a fresh attack. The case of Charles Drummond, which is given in detail at page 205, may indeed be allowed to stand in the future as the type example of a group. It exemplifies the recurrence during ten years of an eruption of centrifugal vesications on hands, feet, ears, and knees. The eruption has been apparently restrained by a long course of arsenic. Last Tuesday (March 17th) I produced this lad at one of my Clinical Lectures, and demonstrated his condition during an attack. His case might be claimed by nosologists as one of "erythema iris," or "erythema multiforme," but I am concerned for the present to neglect the name in the endeavour to realise something as to the real nature of the malady. It is to be noted that the eruption on the extremities is, in all its attacks, preceded by herpes labialis.*

* This labial herpes may perhaps be no real part of the malady, but the result rather of the slight rigor which ushers in the erythema. It is well known to follow rigors however caused.

It may be convenient to state the facts of this case in the schedule form.

Statement of the case of Charles Drummond in schedule form.

YEAR.	AGE.	DETAILS.
1887	11	November: His first attack. The mouth was affected, and hands and ears. Temperature high, and in bed ten days.
1888	12	Easter: His second attack; much milder than the first; same parts affected. Third attack in August.
1889	13	A fourth attack (in the spring).
1890	14	Attacks occurred in April, June, August, and November. In the latter he was shown at Lecture. Arsenic prescribed.
1891	15	May: A rather severe attack after a chill.
1892	16	No attack during this year, but herpes labialis often threatened.
1893	17	November: He attended a Lecture to illustrate cure by arsenic. No attack for nearly two years.
1894	18	Still taking arsenic, and free both from herpes labialis and erythema.
1895	19	Almost wholly well. A slight threatening has occurred. He has left off the arsenic.
1896	20	In March another attack just like the others. Mouth and knees affected, as well as hands, ears, and feet. (Again shown at Lecture.)

The lad who was the subject of this case was of fair complexion and, although in good general health, of very mobile circulation. He easily flushed, and easily became very pale. Sometimes his cheeks would be red and lips almost white. When liable to the eruption he seldom took an ordinary cold in the head.

CASE III.—*Three attacks of Cheiro-pompholyx, during ten years, in a healthy man.*

The following case is one which I took to one of my demonstrations as a good example of Cheiro-pompholyx. A healthy man, aged 33, had had three definite attacks. The first was about ten years ago and was not a severe one. Two years ago, however, he had had a very severe one. It

was attended by what he called "big, watery bubbles" on the hands and lasted six weeks. His present attack had existed more or less for six weeks, but had undergone acute aggravation a few days before I saw him. His fingers and the hands generally were covered with bullæ and vesicles. He could give us no clue to the cause of the attacks, excepting that they occurred when he was much worried. When the inflammation passed off, his skin always resumed a perfectly healthy condition. It did not appear that his hands were at all susceptible to exposure to sun or wind. He said that during the previous summer he had been accustomed to row a great deal, and had sailed in a yacht, using his hands for everything, and that they were never in the least irritated. There was not the slightest tendency for the disease to pass into eczema. No other parts than the hands had ever suffered. A week after I had demonstrated the case to my class the hands were almost well. He had suffered much business anxiety just before the attack. The only peculiarity as regards his skin which he recognised was that it very easily became what he called "juicy." He had no difficulty as regards perspiration, and said that his hands in particular would sweat easily. He was sent to me by Dr. Ludwig.

CASE IV.—*Two attacks, with an interval of five years, of a ringed lichenoid eruption resembling Ringworm, occurring over the whole body and limbs of a healthy girl—Mother liable to Psoriasis.*

On March 2, 1889, Dr. Macpherson, of Finsbury Park, sent to me a girl aged 11, in whose case I diagnosed pityriasis rosea. It had begun about Christmas on her neck and breast, and was fast fading. The rings were breaking up, and only some dark ill-margined stains remained. It had, I was told, at first been very copious, and had looked like ringworm or lichen circumscriptus. There was little or no desquamation, with the exception of a single patch on one eyebrow. I saw this child on this occasion but once. The remains of the eruption, as foretold, quickly disappeared, and

for five years following the patient had excellent health and a perfectly clear skin. In July of 1894 she had another attack of the eruption, and on the 26th of that month she was brought to me covered with a lichenoid rash. It was arranged in spots, small patches, and rings, varying in size from a shot to a shilling. The spots were at first merely congested, but as they spread into rings they became rough, especially at their borders. They had been very irritable, and some of them felt sore and tender. They were very plentiful on the trunk, neck, buttocks, and limbs. None on the face, nor on the elbows or knees. The diagnosis of lichen planus was suggested, but nowhere could any polished patches be detected. The eruption was, however, of very recent development, its first spots having been observed on the arms only three weeks before I saw her. She was now fifteen, and well developed.

A point of great interest in this case is that the girl's mother had been from the age of ten liable to psoriasis attacks. They had occurred over and over again after confinements, and after attacks of quinsy. The eruption had affected the usual positions, and had often appeared to be easily cured by arsenic.

The question arises in this case as to whether the eruption was in reality more nearly allied to pityriasis rosea or to lichen planus. Both of these maladies may disappear spontaneously. Respecting the latter, it is well known that after long periods of perfectly sound skin returns of eruption may take place. I do not know anything as to second attacks of pityriasis rosea. The sequel will probably give the diagnosis.

CASE V. — *Accurate bilateral symmetry — Two Brothers affected — A recurring Herpetiform eruption of very transitory duration—The face, trunk, and upper parts of limbs affected—Definite effect of Arsenic.*

The case of a youth named T——, or rather, I should say, of two brothers of that name—is of great interest in reference to this subject. At the time that I write I have seen but one of the brothers, but his mother assures me that a younger

brother suffers in exactly the same manner, and I am promised an opportunity for inspecting him. Should the statement prove correct, a very important item of evidence will be furnished, since it will be made clear to demonstration that these peculiar forms of dermatitis depend upon peculiarities of original organisation.

The youth whom I have seen is sixteen years of age, a pale, rather pasty lad of fair complexion. He is in general appearance so like the boy Drummond that they might be mistaken for each other.

Master B—— T—— was first brought to me on June 14, 1893. I was then told that he had been the subject of an eruption for eight months, which varied much in severity at different times, but never wholly left him. It was said to have begun, quite suddenly, by what were described as water-blisters under his arms, on his shoulders, and on his buttocks. It had been very irritable, and he had scratched much. The legs and feet, forearms and hands, had throughout been free. No cause could be assigned. He had not been recently vaccinated, nor had he had chicken-pox. When I saw him he had only the remains of the eruption and the marks of scratching. The symmetry, definite localisation, and tendency to spontaneous subsidence and recurrence made me think that it must be an ally of herpes, and I prescribed Fowler's solution in three-minim doses three times a day.

Three months later I saw him for a second time, and was told that since taking the arsenic he had been almost wholly free from his eruption, but not without many threatenings of relapse. He was in excellent health. I then wrote in my note-book: "The arrangement of the eruption clearly suggests herpes, but is said to be always symmetrical. His mother describes 'water-blisters, as if he had been scalded.'"

On the 26th of December I had an opportunity for the first time for seeing the lad during an attack. I was told that he had continued the arsenic, and had remained quite free until five days ago. Then, without warning of any kind, the blisters began to form again, and came out pretty freely in their usual positions. Although so short a period had elapsed, the vesications had, when he came to me, all given way, and only super-

ficial abrasions and portions of very thin peeling epidermis were to be seen. From the size of the latter, I should judge that some of the vesications had been as large as horse-beans. They had evidently been covered by an exceedingly thin layer of epidermis, the remains being not so thick as the thinnest tissue paper. The parts affected were the shoulders, the front of the neck, the face, and the hips. On all these parts the vesicles had been grouped and abundant, and arranged not unlike herpes. On the whole of the back were small scattered spots and a few such also on the upper arms and thighs. Below the elbows and knees there were none, and never had been any. The vesications were clearly more delicate, more superficial, and more transitory even than those of herpes. The lad assured me that he had not felt ill in the least before the attack, and the only inconvenience had been burning and tingling. He did not think that he had caught cold. He had recently been apprenticed to a linen-draper. The shop was draughty, and he had repeatedly had colds, with the usual symptoms of running from the nose, &c., but with no eruption.

It was on this occasion that I was first told that a brother of my patient suffered in a similar manner. He was eighteen months younger, being only 14, but the eruption had begun at exactly the same age in both and had affected precisely the same regions. The younger boy had his first attack in the spring of 1893, and since then so many that although they were distinctly paroxysmal, he had rarely been long free. Recently he had been taking his brother's medicine, and the intervals had been more marked.

CASE VI. — *Recurring catarrhal attacks of Pustular Ophthalmia.*

Miss H——, a hysterical girl aged 15, from Sidcup, came to consult me in April, 1892. She was of very fair complexion and delicate skin; and suffered from recurring attacks of pustular ophthalmia, evidently of a catarrhal nature. Whilst living for two years in London she had remained free as regards the eye affection, but had suffered

from headaches. She was also liable to sore spots in the nose, and had had a gland abscess in the neck. She loved hot weather, and was troubled much by cold hands and feet. I was told that every evening she became feverish. One of her brothers was subject to recurring styes, and a sister had had a glandular abscess.

ON PARAPLEGIA AFTER SYPHILIS.

In the paper with which the discussion at the Medico-Chirurgical Society on "Affections of the Nervous System in the Secondary Stage of Syphilis" was introduced, I gave a list of fifteen examples of paraplegia occurring within the first two years. For the sake of brevity these cases were given only in the form of Case-Headings. This very condensed method of reporting may be made to display the principal features of a case, and it is at any rate very preferable to a bare statistical statement of personal experience. The time at my disposal on the occasion referred to did not allow of considering the whole group of cases in which paraplegic conditions follow syphilis. My only object was to prove that a majority of them are met with during the early or secondary stage. Incidentally, however, I advanced the statement that when paraplegia is met with at later periods it not unfrequently presents a somewhat different group of symptoms and runs a somewhat different course. I now recur to the subject in order to add such clinical facts as I possess which bear upon this assertion, and to give the reader a summary as nearly complete as I can make it of my experience in reference to paraplegic conditions as they occur in those who have suffered from syphilis. Most of my cases I must be content to record in the form of headings, but a few of the more important will be given at greater length. It will be convenient, in order to bring the whole under review together, to reprint the fifteen cases to which I have alluded, and to thus allow the additional ones to constitute with them one series.

CASES OF PARAPLEGIA OCCURRING WITHIN THE FIRST TWO YEARS AFTER PRIMARY SYPHILIS.

CASE I.—*Syphilis at age of thirty—Almost absolute paraplegia as regards sensation and motion five months later—Sphincters affected—No record of result.*

CASE II.—*Syphilis in a man of forty-eight—Paraplegia six months later, with deafness of one ear—Almost complete loss of motion in one lower limb—Great anæmia—Upper limbs threatened—Almost complete recovery under mercury.*

CASE III.—*Syphilis in a man—Complete paraplegia six months later—Symptoms severe and persisting for five months—Partial recovery under prolonged treatment.*

CASE IV.—*Syphilis in a man—Paraplegia seven months later—Complete loss of motion in legs, with great exaggeration of reflexes—Anæsthesia—Incontinence of urine—Sequel not known.*

CASE V.—*Syphilis in a man of twenty-two—Premonitory symptoms of paraplegia eight months later—Sudden development to completeness—Anæsthesia of legs—Paralysis of bladder and rectum—Symptoms persistent for eight months—Gradual improvement up to a certain point—Power of walking not regained, and unable to dispense with the catheter.*

CASE VI.—*Syphilis in a married woman aged thirty—Nine months later paraplegia, both as regards sensation and movement of lower extremities—Sphincters affected—Recovery, but walking power remaining feeble.*

CASE VII.—*Syphilis—Inefficient treatment—Marriage at end of tenth month—Paraplegia two months after marriage—Right lower extremity more severely affected than left—Exaggerated reflexes—Recovery under specifics (man of forty-five).*

CASE VIII.—*Syphilis—Treatment by diet only—Long persistence of eruption—Paraplegic symptoms in the eleventh month—Loss of sensation and inability to stand—Incontinence of urine—Upper extremities threatened—Improvement under specifics, but report incomplete (man of twenty-seven).*

CASE IX.—*Syphilis in a man—Followed by paraplegia at the end of twelve months—Pain in back—Weakness and numbness of legs, especially the left—Recovery fairly complete.*

CASE X.—*Syphilis in a man of twenty-six—At end of sixteen months paraplegia—Absolute loss of sensation, partial loss of motion in the lower extremities—Sphincter paralysis—Recovery almost perfect.*

CASE XI.—*Syphilis in a man of thirty-nine—Paraplegia eighteen months later—Partial recovery—Relapse eight months later, with very severe pain in the back—Paralysis of bladder and rectum—sensation not materially affected—Partial recovery.*

CASE XII.—*Syphilis in a married woman aged thirty-eight—Eighteen months later sudden onset of paraplegia, involving sphincters and lower extremities—Recovery enough to walk a few steps with the aid of sticks—Sphincters left weak.*

CASE XIII.—*Man of twenty-nine—Paraplegia eighteen months after—Primary syphilis, preceded by pain in the back and bladder symptoms—Partial recovery.*

CASE XIV.—*Syphilis in a man of twenty-six—Treatment delayed until secondary stage was fully developed, and subsequently inadequate—Spinal symptoms in twenty-second month, whilst still taking mercury—Pain in back and numbness of both lower extremities—Numbness in one ulnar nerve region—Gradual recovery.*

CASE XV.—*Syphilis in a man of fifty-four—Slowly developed paraplegia, with incontinence of urine, in twenty-fourth month.*

In the paper to which I have referred the following summary of these fifteen cases was given:—"The cases above given are arranged according to the length of the interval between the primary syphilis and the paraplegia. In the first three this interval was about six months; in 4, 5, 6, 7, 8, and 9 it was between six and twelve months; in 10, 11, 12, and 13 it was between twelve and eighteen months; whilst in 14 and 15 it was just within the two years. In Cases 1 and 4 I do not know anything as to the ultimate result, having seen the patients only during the acute state of the paraplegia. None of the cases ended fatally, and, with the exception of the two mentioned as not followed up, all the patients made a more or less complete recovery. In none could the recovery be said to be complete, whilst in several the patient only just regained the power to walk, and still continued to suffer from inconvenience as regards the bladder and rectum. With one exception no tendency to relapse was observed, and my notes in most of the cases extend over several years after the attack. In the exceptional case (No. 11) a relapse of paralysis occurred eight months after partial recovery, but again partial recovery resulted. This case had the peculiarity that sensation was not materially affected. It was one of those in which the paraplegia did not commence till eighteen months after the syphilis. Most of the cases were uncomplicated by other lesions, but in Case 2 there was deafness of one ear, and the upper extremities were threatened. In Case 8 also the upper extremities were threatened. In Case 14 there was numbness in one ulnar nerve region. With these exceptions it is to be understood that the cases were much alike in being attended by loss of both sensation and motion, with fair symmetry in both limbs, and with paralysis of the bladder and rectum. In all the cases when the condition was advanced the patellar reflex became much exaggerated. I cannot speak definitely as to this symptom in the early stage, but in the more acute class I believe it was for a short time almost lost. With scarcely an exception the patient had suffered from definite and even severe secondary

symptoms, and the treatment had been more or less inadequate. It is to be admitted, however, that several, or perhaps most of them, were actually under treatment by specifics at the time the myelitis set in, and that but few were at the time the subjects of any severe affection of the skin or mucous membranes."

The following are my additional cases :—

CASE XVI.—*Syphilis in a woman of thirty-six; mercurial treatment and apparent recovery, but with persistence of a dusky, ringed eruption on forearms—Paraplegia commencing one year after the primary symptoms—Gradual development during three weeks—Increase of reflexes from the first—Retention of urine.*

I believe that this case became complete after I saw it, and ended in death some months later, but I have no exact information.

CASE XVII.—*Constitutional Syphilis after "gonorrhœa" without external chancre—Irregular treatment—Paraplegia at the end of one year, with paralysis of bladder and bowel—Very severe pain in the back—Incomplete recovery—Legs still slightly numb, but walking power good—Able to play cricket, but impotent.*

CASE XVIII.—*Partial paraplegia in the second year after Syphilis; optic neuritis of one eye only six months later —Possible influene of a blow on the head—Knee-jerks normal—Patient in excellent health a year later, but almost blind in one eye.*

CASE XIX.—*Paraplegia in the second year after Syphilis, preceded by very severe pain in the back—Sphincter paralysis; treatment delayed—Incomplete recovery without any tendency to relapse.*

CASE XX.—*Syphilis in a man of twenty-nine; short mercurial treatment—Bladder symptoms eighteen months later—Incontinence of urine, with slight increase of patella-reflex, but no definite paraplegia—Incontinence permanent, four years later, in spite of much treatment.*

CASE XXI.—*A patient named A——, whom I saw in the St. Thomas' Home with syphilitic paraplegia, regained control over his bladder, but had great rigidity of his legs—Secondary descending lateral sclerosis was diagnosed—He had cystitis and a bedsore—He returned to his home at B——, and I have no further information.*

I am almost sure that this was an early case, and of the usual form; but for the present I have mislaid my notes on these points.

CASES OF PARAPLEGIA OCCURRING MORE THAN TWO YEARS AFTER PRIMARY SYPHILIS.

CASE XXII.—*Paraplegia two years after Syphilis, and six months after marriage—A prolonged attack, gradual recovery—Loss of sex-function—No relapse during twenty-six years.*

CASE XXIII.—*Syphilis in a man of thirty-two—Rupial ulcers—Paraplegic symptoms in the third year—Pain in dorsal spine—Loss of knee-jerks—Failure of sexual function—Partial recovery—Ataxy threatened.*

CASE XXIV.—*Syphilis in a man aged 22—Brief treatment—Incomplete paraplegia in the third year—Partial recovery.*

CASE XXV.—*Syphilis; two years' treatment; marriage three years afterwards—Paraplegia commencing by pain between the shoulders and anæsthesia of the right lower limb, and subsequently complete in both—Sphincter paralysis and exaggerated reflexes—Almost complete recovery under specific treatment.*

CASE XXVI.—*Paraplegia in the fourth year of Syphilis—Sudden development—Incomplete recovery—Entire loss of the sexual function.*

CASE XXVII.—*A man from the Throat Hospital, with tertiary ulceration of his nose and palate (severe), gave the history that he had had Syphilis five years ago, but*

without secondary symptoms, and that three years after the chancre he had been laid up in bed for some months with paralysis of his legs and incontinence of urine and fæces—From this he had almost wholly recovered and was now, two years later, able to walk well—No further notes.

CASE XXVIII.—*Syphilis in a man of thirty-three—Severe ulceration of throat; much iodide given—In third year loss of bone from the nose, and very severe headache—In fourth year a fit, followed by temporary aphasia and extreme restlessness—In fifth year confined to bed with almost complete paraplegia and incontinence of urine and fæces—A second fit, slighter, a few months later—Recovery so that he could walk five miles, but with tottering gait—Debility and restlessness—Alcoholism—During the paraplegia the upper extremities were slightly affected, and his speech was thick—Patella-reflex not recorded.*

CASE XXIX.—*Syphilis in* 1888—*Paraplegia in* 1893—*The attack occurred by a sudden stroke, without any premonitory symptoms whatever—It was possibly cerebral, but he did not lose consciousness, and declared that all that he lost was the use of his legs—Double syphilitic sarcocele one year before the attack—Almost perfect recovery, with excessive patella-reflex.*

CASE XXX.—*Syphilis at the age of twenty-seven—Ulcers on the legs in the seventh year—Incomplete paraplegia in the eighth year—Recovery, with power to walk.*

CASE XXXI.—*Syphilis æt.* 24—*No early treatment—Iritis—Two years' good health—Severe cerebral attack in fifth year—Vomiting, left hemiplegia, optic neuritis—Complete recovery—Incomplete motor paraplegia, gradually developed, in ninth and tenth years—But little defect of sensation—Exaggerated patella-reflex.*

"Spinal Sclerosis" was diagnosed.

CASE XXXII.—*Remarkable persistence of Syphilis in spite of vigorous and continued treatment in a man of twenty-nine—Symmetrical deafness in the third year; partial recovery of hearing—Diplopia, followed by paraplegia in the tenth year—Partial recovery.*

CASE XXXIII.—*Syphilis in a man of thirty—Gradual loss of sexual power from the sixth year, and subsequently gradual development of partial paraplegia in the tenth year—Knee-jump excessive; anæsthesia not noted—Incomplete notes.*

CASE XXXIV.—*Paraplegia in a young woman who had formerly, ten years ago, had Syphilis, and who had recently drunk spirits freely—Rather gradual onset, with obscure symptoms—Retention of urine and paralysis of the sphincters—Cerebral excitement (possibly from alcohol)—Partial recovery of sensation in the lower extremities, but without return of muscular power.*

CASE XXXV.—*Syphilis from finger chancre—Mercurial treatment too short—Hemiplegia in the third year—Recovery—Paraplegia gradual and incomplete, with impotence, in twelfth year.*

CASE XXXVI.—*Syphilis in a man of thirty—In the fourth year an attack of vertigo and inability to stand, which passed off in twelve hours—Three years later sarcocele, and subsequent liability to recurring herpes on the buttock—In the twelfth year two attacks of severe pain in the back, ending in partial paraplegia—Limbs feeling dead, and sensation defective—Inability to stand or to micturate—Recovery under treatment by iodides almost complete.*

CASE XXXVII.—*Syphilis fifteen years ago, æt.* 37—*Liability to morning sickness exactly like that of pregnancy—Defective power of bladder, resulting in complete retention—Defect in co-ordination of the lower limbs, resulting in inability to walk or even to stand alone, sensation and*

muscular power being still perfect — Diagnosis (Dr. Buzzard) disease in the cerebellum—Remarkable tolerance of mercury; complete recovery under its use.

CASE XXXVIII.—*Syphilis sixteen years ago—Twelve years of good health—Gradual onset of paraplegia, with very great pain in the lower part of the back—Unsymmetrical progress—Control of sphincters not lost—Great improvement under the iodides, but never able to walk.*

CASE XXXIX.—*Syphilis at age of twenty—Married three years afterwards—Healthy family—Complete recovery from Scrivener's palsy—Liability to profuse perspiration, followed by a nervous illness—Failure of left leg, with great exaggeration of reflexes—Failure of the other, with similar symptoms—Much jumping of legs—Absence of pain—Conditions stationary for six years.*

CASE XL.—*Syphilis in a married woman—Alcoholism —Paraplegia complete—Cirrhosis of liver?—Death in September*, 1881.

CASE XLI.—*Syphilis in* 1868 — *But little treatment — Good health for six years—Marriage—An attack of "spinal paralysis" in the ninth year—Not symmetrical —In bed some weeks, and recovered — Subsequently bladder troubles and gradual development of tabes.*

In this case the attack of spinal paralysis was quite sudden (a stroke), and in this differed from the cases described as occurring in the earlier stage of syphilis.

CASE XLII.—*Slowly aggressive Paraplegia (during five years) — Muscular development still good — Remote Syphilis—Loss of Sexual function.*

The case of Mr. D—— offers a somewhat exceptional form of paraplegia, the peculiar feature being that it had been insidiously aggressive and remained incomplete, and that it had never been attended by pain in the back. Mr. D——. was (in 1891, when he consulted me) a tall, stout, exceed-

ingly powerful man. He was forty-one years of age when I saw him, and when he had been for five years the subject of his disease. He was married to a wife younger than himself, and they had no children. Ten years ago he had suffered from syphilis. From this he had apparently quite recovered, and unless his paraplegia was of that nature he had had no reminders. The history of the onset of the paraplegia was as follows. It began quite gradually with what Mr. D—— called "sense of contraction in the backs of his thighs." The first thing that he remembered was that he could not rise easily from his seat unless he could get hold of something. Next it began to drag the left leg, and would wear out the toes of his boot. A few months later he dragged the right toe rather than the left. During the early stage his toe-nails, especially those of the great toes, became, as he said, "black," and some of them fell off. He now went into the S—— Hospital, under the care of Dr. D—— M——, and was an in-patient there for six months. After this he consulted Dr. Ross, of Manchester. When I saw Mr. D—— he was just able to walk if he might take hold of any one's arm, but he supported himself very insecurely. Yet his lower extremities were large and muscular. He admitted that the sexual function had been in abeyance some years. He denied that he had ever suffered from pain in the back, but complained much of having had great discomfort.

CASE XLIII.—*Paraplegia gradually developed in the ninth year after syphilis—The bladder first affected—Exaggerated reflexes—Paraplegia complete—Death from hæmaturia one year after the onset of paraplegia—Autopsy—No macroscopic changes.*

In the case of Mr. B——, a young man seen with Mr. Groves, of Woodford, the paraplegia began about a year before his death, and eight years after complete syphilis. He had been quite well in the interval, and accustomed to athletics. Inability to empty his bladder was one of his first symptoms. The paraplegia developed somewhat irregularly, but finally became complete, and was attended by great

reflex irritability of the limbs, which caused much suffering. In connection possibly with the use of the catheter, hæmaturia occurred, and of this he died. I obtained a post-mortem, and removed the cord, which showed no macroscopic changes. The microscopic report has been lost.

The following are more detailed notes of this interesting case. I much doubt whether the paraplegia had any connection with syphilis.

I saw Mr. B—— with Mr. Groves, of Woodford, in the summer of 1880. He had then been confined to his bed for more than six months by increasing paraplegia. He needed the use of the catheter several times daily, and was quite unable to stand. He was, however, still in the habit of raising himself into a sitting posture on the close stool, but in doing this produced much pain in his back. He had been for some time under the treatment of Mr. H. R. Stevens, of Finsbury Circus. He could use his hands well, and had no cerebral symptoms whatever.

In proceeding to investigate his case, I was informed that eight years ago he had suffered from complete syphilis. Since then he had enjoyed excellent health, and had not had any reminders. He was a young man of social habits, and had mixed much in company and lived "fast" in all directions.

His illness dated from June, 1879 (about twelve months), when, at some athletic sports, he strained his back in throwing a hammer. Very shortly after this (a few days ?) his left leg failed, and he became suddenly unable to empty his bladder. A catheter was used.

Three weeks later he went to Eastbourne, and at that time could walk a hundred yards and could pass his water, but was unable to use any force in micturition. After this he went to Yarmouth, and there recovered so far as to be able to walk a mile at a time. It was still his left leg that was chiefly or alone affected.

From this time he never regained the power of "forcing" in making water. In August he went a voyage to the Cape, being still able to walk a mile or so. At the Cape he walked too much, and rather suddenly one day, after lifting a box, his right leg failed. He returned home able only to walk about three hundred yards, both legs being affected, and the left being the worse.

In October he consulted Mr. Bryant, who ordered iodide of potassium. In December he put himself under Mr. Stevens' care. He was steadily getting worse, and in January he became confined to his bed. He took iodide and bichloride of mercury, but not regularly.

I visited Mr. B—— a second time with Mr. Groves on September 30th. It was seven weeks since our former visit, and in the interval he had been kept strictly to bed and had taken mercury to slight salivation. No improvement had resulted. I found sensation in left

foot so defective that he could scarcely feel at all. In the right he could feel better, but not at all well. He could scarcely move either, but was exceedingly annoyed by reflex movements. Thus any touch would cause the legs to be drawn up. When I pressed my hand on his sole under the ball of toes it had the effect of throwing his foot into a state of constant to-and-fro movement (as if treading a wheel very rapidly). Sensation was very defective in the abdomen, and when I pulled his pubic hairs he could not feel it at all on the left side, and only indistinctly on the right. On the lower parts of the chest he could feel well Tickling of the soles produced reflex movements immediately, but almost without sensation.

He could not void urine at all, but on one occasion, when the bladder was distended, it had escaped involuntarily. An indiarubber catheter was used by himself four or five times a day. His bowels were usually costive, but if relaxation was produced he was quite unable to retain his feces. He was very flatulent. The use of the catheter always made his legs quiver. Formerly he had suffered from shoots of pain in his back, but from these he was now free. I urged perseverance with mercury.

On October 19th Mr. Groves wrote to me that "the spasmodic muscular contraction of the legs was almost constant."

I saw Mr. B—— for a third time on October 22nd. This time it was on account of profuse hæmaturia. He had continued to use his flexible catheter without any difficulty, when suddenly a quantity of blood had appeared in the urine. At first it was in clots, and very difficult to get away, but subsequently it had been mixed with the urine. For some days past he had been suffering exceedingly from constant drawing up of his legs. The slightest touch would cause them to draw up, and any attempt to control them only made it worse. He was often kept awake the whole night by these spasms.

We gave ergot of rye, and applied cold water to the pubes, but the bleeding continued, and he sank from it a few days later (October 25th). The tendency to spasm diminished as he became weakened by the loss of blood.

Of late he had lain constantly on his back, and at one time bed-sores had been threatened, but the tendency ceased when he was put on a water-bed.

With Mr. Groves I made a post-mortem examination on October 26th. The spinal cord was removed, and nothing was observed indicating disease. It was not obviously softened. It was brought away for microscopic examination.

The bladder was much distended. It was much congested, and its mucous membrane was very dark. Its walls were soft, and tore readily. Its contents consisted of urine mixed in large quantity with blood. Both kidneys were large, and the right was much congested and dark in patches. Very possibly it had been the source of hæmorrhagic draining.

CASE XLIV.—*Spinal symptoms in the eighth year of syphilis — Staggering gait — Exaggerated reflexes — Defective articulation—Difficult respiration—No benefit from treatment—Death from accidental poisoning.*

In a case which I saw in Dublin a man had had complete syphilis in 1881. In 1889 he had double vision, and soon after noticed that his toes used to catch in walking. After that he had staggering gait, and used to walk with his legs wide apart and body thrown forwards. No sphincter trouble. Ultimately entire loss of power to walk, reflexes excessive, some defect in articulation, no optic neuritis, general prurigo of skin, and a certain degree of general hyperæsthesia. Death from an overdose of morphia. This patient had probably been excessive in sexual intercourse and in the use of stimulants. One of his symptoms was inability to breathe in the recumbent position, so that he was for some time before his death obliged to sleep in his chair.

(*To be continued.*)

SPONDYLITIS DEFORMANS.

THE term Spondylitis deformans is applicable to advanced cases of anchylosing osteo-arthritis affecting the spinal column. The adjective denotes the peculiar arching and fixation of the spine which results. Whilst rheumatoid arthritis of the vertebral joints is probably common, well marked examples of general anchylosis of them are exceedingly rare. Our English literature contains very little respecting this malady. One of our most modern dictionaries does not even mention the word.

Mr. Lawrence, in a paper in the Medico-Chirurgical Transactions for 1827, makes brief mention of a number of museum specimens in which the cervical vertebræ were anchylosed. Some of the cases probably belong to this category, but in none was the condition traced to the dorsal spine. Dr. Robert Adams records the post-mortem changes found by Professor R. W. Smith, and described by the latter thirty years ago to the Pathological Society of Dublin. In the London Pathological Society's Transactions for 1877 the late Dr. Hilton Fagge described a case very fully, and illustrated it with woodcuts. He thought the disease unique, and gave it no name. His patient was a young man of thirty-four who died with bronchial dilatation, &c., a few days after his admission into Guy's Hospital. He had been bent double, so that his chest touched his abdomen, and his spine and ribs had been quite fixed. The autopsy showed bony anchylosis of the arches and spinous processes and of the rib joints, but the inter-vertebral substance and the anterior ligament were not ossified. The neck was not examined, but the whole of the dorsal region was involved

PLATE CXXXIII.

SPONDYLITIS DEFORMANS.

This Plate is copied from a photograph of a specimen which is in the Dupuytren Museum in Paris. It was one prepared by Dupuytren himself. The conditions shown are complete ankylosis between the bodies of all the vertebræ, and of all the smaller joints, including those of the ribs. There is the usual posterior curve, which is strongly marked. The label attached to the specimen is as follows:—"Ankylose périphérique avec cyphose générale de toutes les vertèbres de la colonne vertébrale de nommé Séraphin. 652A. Dupuytren."

West, Newm

and completely fixed. One hip was anchylosed. Dr. Fagge comments on the absence of the osteophytic growths common in osteo-arthritis. Unfortunately but few facts are recorded as to the patient's history. He had been ill four years, and his back had been gradually getting stiff and bent.

The first English case which I find described under the same "Spondylitis deformans" is one very ably investigated by Dr. Allen Sturge, and brought before the Clinical Society in 1879. The patient was a man of twenty-six, in whose family there was a strong history of arthritic maladies. One brother had suffered much from gout in his hands and feet, another was the subject of chronic rheumatism, having previously had rheumatic fever. Their mother suffered much from rheumatism. He dated his malady eight years back, and said that it began by attacks of severe pain. He had just previously suffered from a gonorrhœa, which he declared was spontaneous. At first his back and neck became stiff, and he was unable to stand up straight, but subsequently he recognised that his ribs were becoming fixed, and that he could not breathe easily. He had recently had two attacks of iritis called rheumatic. This poor fellow was very much deformed, and, with the exception of his upper cervical vertebræ, his whole spine and all his ribs appeared to be fixed. There was no stiffness of any of the limb joints nor of those of the clavicles.

CASE I.—*Spondylitis Deformans in advanced stage with stiffened hips—Repeated attacks of Gonorrhœa—A previous attack of Rheumatic Fever.*

A very marked instance of this malady in association with hereditary gout and acquired gonorrhœa was under my observation in 1895. Its subject was an officer on a sea-going ship, aged 37. He had suffered at the age of 25 from rheumatic fever. It followed some months after a gonorrhœa, but was possibly in association also with malaria. His inheritance of gout was strong. At the age of 27 he had his second gonorrhœa, after which he con-

tinued to be more or less rheumatic. After a third gonorrhœa, at the age of 31, one hip became stiff, and from this time his rheumatism was steadily aggressive. When I was consulted, both hips and the whole spinal column were stiff. His chest was bowed forwards and his respiration wholly diaphragmatic. He was a spare man, and had lost much flesh and suffered much pain. The smaller joints of his limbs were not affected, and he was able, after a fashion, to walk, and had, with remarkable pluck, insisted, until within the last few months, on going to sea. I felt obliged to dissuade him from making any further attempt, and to give him an unfavourable prognosis. So far as I could judge the whole spinal column, with the exception of the upper cervical vertebræ, appeared to be anchylosed and most of the ribs fixed.

CASE II.—*Spondylitis Deformans in an advanced stage without implication of the limbs—Partial fixation of the Scapulæ.*

Another very severe example of this condition was repeatedly the subject of demonstration at my Clinical Museum in 1894. He was a man of about 36, and was sent by Dr. Stocker from Forest Gate. His respiration was wholly diaphragmatic, none of the ribs moving in the least. The scapulæ also appeared to be becoming fixed, and could be made to move but little. The trunk was bent forwards. The exemption of all the joints of the limbs from arthritic changes was most definite. The man walked well—with his face always looking downwards—and could use his hands well. He was, however, by the fixation of his trunk and head quite incapacitated from his occupation. He was a man of strong form, and by no means extremely emaciated. He denied having ever had gonorrhœa, and there was, I believe, no strong history of arthritis in his family that he knew of.*

* This case has probably been reported in more detail in the *Clinical Journal.*

CASE III.—*Inheritance of Gout in a lady—Nodi Digitorum and Spondylitis Deformans—Three attacks of Jaundice—Bronchocele—Prurigo and Pruriginous Eczema—Loss of flesh.*

Mrs. C——, aged 67, a married lady without children, who had lived for the last thirty years at Brighton, consulted me on October 9, 1890. Seven years before she had a very prolonged attack of eczema and was cured by my prescription. She was still liable to an erythematous eczema which came and went and was worse in the mornings; but at the time of her visit there was little to be seen. She had been using the tar washes which I had formerly prescribed. Mrs. C—— was much thinner than she had been. She easily caught cold, and told me that she could never digest beer. In two years she had had jaundice three times. She was the subject of a large and very firm bronchocele, the right lobe and isthmus being involved. There were very marked nodi digitorum, and her spine was stiff.

On November 20th, when I again saw her, she was better, but the skin was still irritable. In attempting to stoop her hips only bent; the whole dorsal spine appeared to be quite stiff. Her mother was crippled with rheumatic gout, and there was true gout in other relatives.

The two following cases I give with some hesitation; they illustrate the difficulties which often attend the diagnosis of Spondylitis Deformans.

CASE IV.—*Very severe Lumbago—Query Spondylitis Deformans.*

Mr. Ward, of Leeds, brought to me an interesting case in which the diagnosis lay between very severe lumbago and organic disease of the spine. The patient was a very tall and very thin man, by birth a Swede. His attack had begun quite suddenly, about six months before I saw him, with agonising pain in his back, located chiefly in the middle of the sacral region. In this attack he had been

confined for six weeks to his bed, and during most of it had been quite unable to move. If he lay quite still he was, he said, comparatively free from pain, sometimes, he added, wholly so, but the slightest effort to turn on his side produced pain of the most agonising description. If it was wished to apply a blister to the affected part, his attendants were obliged to lift him up and slip the blister under him, for he could not endure any turning on to his side. During this illness the pain, he said, shot down his hips, but it was not attended by any approach to paralysis of the legs. When he got up he was so much emaciated that although six feet high he weighed only seven stones. When the pain was mitigated he gradually recovered his health, but still remained very stiff in the back. It was on account of this stiffness and inability to get quite rid of the pain that he consulted Mr. Ward, by whom he was brought to me. He had previously in Berlin obtained the opinion of Professor Leiden.

When Mr. K—— was stripped, I could find no irregularity of the vertebræ, nor did he experience pain from firm pressure on the loins. He was still so stiff in the lower part of his back that he was quite unable to stoop unless he took hold of something. He said that he could not stoop to the wash-hand basin unless he supported himself by a hand placed forward. With one hand placed in advance he became quadrupedal, and could then bend his body forward at the hips, keeping the spine straight without losing his centre of gravity. He had gained two stones in weight since recovery from his illness, and was now able to get about tolerably well. Neither Mr. Ward nor myself had much hesitation in regarding the case as one of very severe lumbago, and the speculation became of interest as to what structural change had occurred which so completely prevented flexion of the spine. Could it be that this was wholly due to contraction of the fascia and aponeuroses, or was it not more probable that changes had taken place in the joints between the vertebræ. If the latter, however, it is difficult to explain the sudden onset of the attack and its limitation to the joints of the spine, for he had no evidence

of articular disease elsewhere. His back was perfectly straight, showing no approach to the roundness which is usually so characteristic of what is known as spondylitis deformans. I may add that there was no complication of gonorrhœal rheumatism in the case.

CASE V.—*Spondylitis Deformans mistaken for syphilitic disease of the spine—Diagnosis doubtful.*

Mr. J. J——, the mate of a merchant vessel, was sent to me from Yorkshire, November 13, 1894, with a suggestion of syphilitic disease of the spine. He was an intelligent man, fifty years of age. He knew that he had suffered severely from syphilis twenty-five years before, and, to use his own expression, had been "fighting the disease ever since." In other words, he had been almost constantly taking iodide of potassium. On going into his history, however, it did not appear quite certain that what he had experienced had been really due to syphilis. He had nearly the whole time been able to continue at his vocation. He had married, and his wife had borne eight healthy children. He had himself the appearance of good health and, excepting the suffering in his back, had no ailments. What he called "neuralgia" in the muscles of the back and in the bones had been his chief trouble; but he thought there had been some loss of power in the lower limbs and pain and difficulty in walking. Six or seven years before, he had, he considered, decided weakness of the lower extremities. He had always been able to relieve his pains with iodide of potassium. Three or four doses, he said, would very often be sufficient. Hot baths also had always been a great relief to him. His pains had never been worse at night. When he was stripped I found that his whole vertebral column, with the exception of his neck, was quite stiff. He stood with his sternum and epigastrium bulged forwards, the characteristic position of spondylitis deformans; but his spine was almost straight. Over the sacrum there was an ill-defined puffy swelling, which was quite definite, and in this part there had recently been much pain. Near to this on the left side there was a

subcutaneous, hazel-nut-sized, movable tumour, possibly a neuroma. On questioning him as to the degree of weakness in his lower extremities, he admitted that he thought he had walked with difficulty because he was afraid of jarring his back. His knee reflexes were good, and he had never had any difficulty in micturition. There was no strong history of rheumatism in his family, and none at all of gout that he knew of. I could not discover any periostitis of any moment, and my final diagnosis was that he had been suffering throughout from rheumatic arthritis of the spinal joints ending in spondylitis deformans, and not from syphilis.

ON THE RECURRENCE OF INDURATION IN THE SITES OF FORMER CHANCRES.

THE phenomenon of recurrence of induration in the site of an old chancre is a very remarkable one, and worthy of more attention than it has as yet received. I have on various occasions recorded examples of it.* The last two of these will be found so recently as the last number of ARCHIVES, p. 171 *et seq.*; I am, however, induced to recur to the subject now because no fewer than four instances have since then come under my notice. In three of these I had myself treated the patient for his original chancre, and was therefore cognisant of his whole history, and in one of these the interval between the first and the second was no less than nineteen years.

In the chapter on the Relapsing Chancre in my little handbook on Syphilis, p. 121, I have made a statement to which my recent experience has, whilst confirming the rest, furnished some remarkable exceptions. It is that these recurred chancres usually disappear readily under treatment, and are seldom of long duration. In several cases recently under care the induration has persisted for two or three months, and in spite of the vigorous use of specifics. The expression used was "almost always disappear quickly," but in the same chapter I recorded one exceptional case in which the sore was said to have persisted for six months in spite of mercury. In this instance, however, it disappeared in a month under the inunction treatment.

These recurrent indurations have an interest which extends

* My first paper describing the recurring or relapsing chancre will be found in the London Hospital Reports for 1866. In 1868 Fournier published in the "Archiv. Général de Méd." an excellent article on the same subject under the title of the "Pseudo-Chancre induré."

far beyond themselves. They illustrate, in a part well exposed for examination, pathological possibilities which may be of more or less frequent occurrence in structures concealed from view. Their lesson in reference to the possible latency of germinal material is most important and far reaching. In one of the cases which I am about to record I treated the patient in the first instance for an indurated chancre which was somewhat more than usually persistent. After it had disappeared, however, the part remained perfectly sound. The man married and had healthy children. Nineteen years after the first sore another induration formed in its site, which became as hard as a disc of cartilage, and persisted in spite of treatment for many weeks. It was in many respects a simulation of a Hunterian chancre of the most typical kind. Yet it was not a real chancre, and in all probability was wholly local, and contained nothing of the nature of the syphilitic virus. It did not cause enlargement of glands, did not infect the man's wife, and was not followed by any constitutional symptoms. Excepting that the interval between the two was the longest that I have ever known, the case conforms in the facts just mentioned to what has been observed in many others. It is the sameness of the cases which enables us to feel so certain of the facts.

Why should these recurred chancres repeat so exactly the phenomena of the primary one? They are as hard as cartilage, and often as abruptly limited as if a disc of cartilage had been inserted into the tissue. They often even exaggerate the qualities of the hard chancre, and are, like it, not prone to suppurate, but frequently remain for long with an unbroken surface. It seems quite certain that something has been left behind by the original chancre capable of relighting a plastic inflammation of the same type. This something can scarcely be the virus itself, for it never causes infection. It must rather be of the nature of a chemical poison, or, to speak vaguely, of a cell proclivity. That it is consequent on the bygone local inflammation and not on any state of the blood is evident, for the recurred sore is almost always exactly in the site of the original one, and is never attended by any other phenomena of constitutional affection.

A troublesome and long-persisting Chancre—Long course of Mercury—Subsequent liability to Herpes—Recurred induration in seventh year.

YEAR.	AGE.	DETAILS.
1890	32	Syphilis in March. The induration persisted in spite of treatment for some months.
1891	33	Taking mercury.
1892	34	Liable to herpes in the penis, for which I gave arsenic.
1893	35	Taking arsenic. No symptoms except herpes.
1894	36	Under my observation for gonorrhœa. No symptoms of syphilis.
1895	37	Permission to marry, having been three years well.
1896	38	March. A recurred induration in the site of the old sore.

In this instance the recurred induration was a large flattish disc, exceedingly hard and well defined. It ulcerated in the centre and was for a time painful, but never suppurated freely. It behaved in this respect exactly like a primary sore. It resisted treatment for some time. Mercury was pushed until ulceration occurred in the cheeks, and there was severe aching of the jaws. Under this treatment the sore healed soundly, but still remained very hard. Mercury was pushed through April and the greater part of May. Towards the end of the latter it was omitted on account of the pain in the jaws, and the iodides alone continued. The sore had been slowly yielding, and it continued to soften under the latter. By the end of June it had almost wholly disappeared. There had been no other symptoms whatever.

In further illustration of what I have written above I will now give the details of a case to which allusion has already been made. Its subject is a gentleman whom I treated nineteen years ago for syphilis, and who is now under my care again for a recurred induration in the site of the chancre. He has since enjoyed good health as a married man, and been the father of a healthy child. Nothing whatever had reminded him of his syphilis until the chancre

recurred, but he had been long liable to a peculiar form of urticaria, which would be brought out by any local irritation. If his collar had chafed him it would come on his neck; if in bed he rested too long on one side or his nightdress got into folds, there would come blotches of erythema. So troublesome was this eruption on the inner sides of his thighs and knees from the friction of the saddle that he had been obliged to desist from riding. The severity of the eruption has always seemed proportionate to the amount of pressure which had caused it. When it has been caused by pressure in bed it has rarely lasted twelve hours, but when induced by riding it would remain for two or three days. Little or no irritation usually attended it, and no tendency to spreading was ever witnessed. It was always restricted to the region which had been irritated. It had never appeared to be excited by articles of food.

I had an opportunity for inspecting this eruption as caused by resting too long on one side in bed. The affected side, over the hips, &c., was covered by large abruptly-margined patches of deep congestion, of which the edges only were in the least raised. In this absence of thickening they differed from the wheal of urticaria. They disappeared entirely on pressure, and the patient assured me that before evening they would be quite gone. There was a slight tendency to assume the ringed form, but the centre of the patch, although paler than the border, never became quite free from congestion.

It might be doubted whether this urticarial eruption had any connection whatever with the syphilis which had preceded it. Certainly it was not under the immediate control of mercury, for the patient had been taking mercury for his recurred chancre for a week when he showed me a most characteristic attack. It was, however, only since his attack of syphilis that he had been liable to it.

The following statements concern the primary sore and the recurred one.

I attended Mr. B——, in 1877, for his first chancre, and he came to me in 1896, after nearly twenty years of health, with the recurred one. On the occasion of the second he

had been married for five years, and gave me the positive assurance that he had incurred no risk of fresh infection. His statement was supported by the fact that the recurred induration was wholly without ulceration, that the glands were not enlarged, and that it was precisely in the site of the former one. Its character, a disc of cartilage the size of a horse-bean and a quarter of an inch thick, wholly unattended by inflammation or abrasion, left no doubt in my mind that it was not the result of fresh infection. Mr. B—— had been in the interval, with the exception just noted, wholly free from symptoms. He had married with my sanction, and had a very healthy child three years old. The original sore was of considerable size and well indurated, at one period it was ulcerated and very painful. I saw him at intervals for a year, and gave small doses of mercury continuously. He had no definite secondary symptoms either on skin or in throat, but some years later he was under my treatment for sores on the tongue, the exciting cause being, no doubt, smoking.

Over-leaf is a scheduled statement of the case.

An Indurated Chancre which persisted long—Mercurial treatment for one year—Good health subsequently—Marriage—A recurred induration in nineteenth year.

YEAR.	AGE.	DETAILS.
1877	24	August. Contracted a chancre. I saw him in October and ordered Hydr.
1878	25	He took small doses of mercury for a year.
1879	26	Quite free from symptoms.
1880	27	
1881	28	
1882	29	
1883	30	
1884	31	
1885	32	
1886	33	
1887	34	Patches on the tongue from smoking. I gave him mercury again.
1888	35	Quite well.
1889	36	
1890	37	
1891	38	Married with my sanction.
1892	39	Quite well.
1893	40	A healthy child born, which lived and remained well.
1894	41	Well.
1895	42	Well.
1896	43	He came to me with a recurrence of induration.

The original chancre was ulcerated and very painful, and after it had healed under mercury it remained very hard. It was in the under surface of prepuce on the left side. Enlargement of glands in the groin had been observed before the sore.

An instance of absolute suppression of Syphilis, but with recurred indurations after marriage—Treatment begun in sixth week and before any secondary symptoms—No secondary symptoms subsequently—Mercury continued eighteen months—Relapse of the Chancres after too early marriage—Three indurated sores.

YEAR.	AGE.	DETAILS.
1893	28	October. Three indurated clean sores; hard glands; red throat; no eruption. November 2, treatment.
1894	29	Has taken H. cum C. regularly. No secondaries. Loss of flesh and some suspicion of phthisis.
1895	30	March 27. Is quite well.—May. Married.—October. Has now three clean but indurated sores in reflected prepuce, exactly where the first chancres were.
1896	31	The recurred indurations have vanished. He has chloasma, but nothing else.

When Mr. C. came to me in October, 1893, he had three well-indurated chancres in the roll of the prepuce and hard glands in both groins.

On June 24, 1895, he came to me because he was anxious about a certain redness which had come in the reflected prepuce just where the chancres were formerly. He was now married, and his wife had suffered nothing. There was no abrasion and no hardness, simply congestion, and possibly a slight tendency to parchment. He came to me again in July, and it was then hard. He was still taking the pill three times a day (August 30). There was now, in spite of mercury, a disc of induration as big as a sixpence, and like cartilage or three thicknesses of parchment. No ulceration. Mercury was left off in the following March, all traces of induration having disappeared.

Recurred induration in the site of a chancre twelve years after the original sore.

I have seen, just in time to give it a place in this article, another remarkable illustration of the facts under consideration. The patient is a man of about 46 who was under my

own treatment twelve years ago for syphilis. He had a long treatment, but ultimately got well, and has during the subsequent period enjoyed excellent health. During the last few weeks, however, an induration has developed in the sulcus of the prepuce. It is wholly submucous, and is unattended by ulceration or even the slightest congestion, but is as hard as if it were a bar of cartilage. The parts had for so long been perfectly sound that my patient had forgotten the precise site of his original chancre, but there is not much doubt that the new one occupies it. That he is truthful in alleging that there has been no recent risk of infection is borne out by the fact that no abrasion whatever has preceded the induration, and also by the character of the latter, which is deeply placed, and, as stated, without surface inflammation. The absence of gland enlargement may also be quoted in support of the same conclusion.

There can be no doubt that these recurred indurations are to be regarded as tertiary phenomena, and that in this sense they are gummata. Although in most instances their cartilaginous hardness constitutes a feature in which they differ widely from ordinary forms of gumma, yet we must not lay too much stress upon this. Some of these recurred sores are not extremely hard, while some gummata, especially those in muscles, form very quickly, and by their induration and abrupt limitation of outline closely simulate malignant growths.

As an example of a recurred sore which did not become typically indurated, I may quote the following. It is only one among many.

A fungating granulation-mass, simulating cancer, developed in the site of a chancre in the sixth year.

I attended Mr. S—— for a primary sore six years ago. It had somewhat peculiar features, being a very large induration and remarkably persistent under treatment. He took large doses of mercury, and over a considerable period of time, before the sore made any progress. I was obliged to insist on his confinement to the house for some weeks,

an expedient which is very seldom indeed necessary. At length, however, the sore healed and the induration disappeared, but it was not until after more than six months' use of mercury. The drug was continued for six months longer, and no secondary symptoms ever showed themselves. During the present year I have had this gentleman again under treatment for a large, rather firm fungating mass, which has developed in the site of the old chancre. It is as big as a half-walnut, but presents no approach to specific induration.

In a case in which the facts were very similar to those just narrated, the conditions presented by the fungating growth were so like those of epithelial cancer that I felt some doubt as to whether it was right to delay an operation. After a consultation with Sir James Paget we decided to persevere with specifics, and under the continued use of iodides sound healing resulted. I have seen the patient repeatedly during the ten years which have since elapsed, and know that he has remained well.

Induration in tertiary gummata of other parts.

The phenomenon of induration in connection with true tertiary gummata of other parts than the genitals, to which allusion has already been made, is worthy of some consideration. It is often very remarkable in its character. Some of its best examples are met with in gummatous tumours formed in the substance of the tongue. I have recently been attending one such, in which the condition was much as if a toy marble had been embedded in the organ. There was no inflammatory action, and, excepting that the surfaces above and below were slightly elevated, there was nothing to disclose its presence until the finger was used.

No. XLVI.—*Suppurative destruction of a terminal phalangeal finger joint from gouty Arthritis.*

In the case of Mrs. H——, aged 70, an old lady who, considering her years, was in very good health, suppuration occurred in the last joint of one of her fingers. When she was brought to me by Dr. Hingston Fox, there were open sinuses communicating with the joint, and the bones grated on being pressed together. There was very little swelling of the soft parts. The attack had commenced with very severe pain. She was the subject of last-joint arthritis, and there was gout in her family. The synovitis, which had ended in suppuration, had apparently commenced spontaneously and was probably of the nature of rheumatic gout. I advised that the joint should be fixed in a splint and allowed to anchylose.

No. XLVII.—*On the frequency of sub-acute gouty Arthritis of the finger-joints in those who inherit Gout.*

In connection with the preceding case it may be remarked that although the formation of Heberden's nodes and other rheumatic affections of the finger joints are not often attended by acute arthritis, yet that the latter does every now and then occur. We may even have sub-acute gout in one of the last joints and limited to it. Now and then, as is well, gouty arthritis will run on to suppuration. When it does so, the distinctive features of difference from most other forms of joint disease are still maintained. It almost always begins with very severe pain, and after the

pus has been evacuated shows a remarkable tendency to refrain. The inflammation rapidly subsides, the sinuses heal, and anchylosis results without any persistence of swelling.

A gentleman, aged 67, of rheumatic family, and who had himself had many threatenings of gout, consulted me on account of pain in one finger end. He could not bear to touch anything. The whole of the finger end was swollen and tense, and at first I much feared a periosteal whitlow. On careful examination, however, I made out for certain that there was no tenderness on pressure on the bone, and that the pain came only when the joint was moved. A week's treatment on the lines of gout was sufficient to wholly remove the inflammation. It may be added that the patient had some small nodi digitorum on other finger joints.

I have seen several cases similar to the above, in which those who had nodi digitorum suffered from transitory, rather sharp attacks of inflammation of single joints. Sometimes these subside quickly without leaving any stiffening, but in others they result in permanent crippling, with increase of the bony outgrowths.

No. XLVIII.—*Raynaud's phenomena in association with Inherited Gout.*

An example of lifelong liability to Raynaud's phenomena in association with gout was brought under my notice in the person of a lady of upwards of 70. She was spare and pale; she had lost the tops of several of her fingers, but not, as was asserted, by gangrene, but by abscesses from which chalk-stones had been discharged. These concretions she would pick out herself. In confirmation of the opinion that they were really chalk-stones, she showed me one under the skin in the middle of one finger, which certainly appeared to be of that nature. She had none in her ears, nor was the family history of gout very strong. Mrs. McL—— told me that from girlhood she had never been able to put her hands in cold water, as the fingers would infallibly die. As she had got older this

especially affected one knee, in which it had been attended with most severe pain, and in which it seemed likely that permanent stiffness would be the result. Our patient was only nineteen years of age; tall, thin, and of very dark complexion. He was the nephew of a personal friend, and it chanced that I already knew his family history. There was hereditary gout on both sides.

CASE III.—*Anchylosis of the knee from Gonorrhœal Rheumatism—Family history of Gout.*

Mr. D——, aged 34, a tea-planter from India, consulted me in February, 1892. In the previous April he had contracted gonorrhœa in London. An acute rheumatic attack followed on May 10th, in which only the knee and ankle suffered, the latter only slightly. The wrists were threatened for a few days. The temperature rose to 104° and the pulse to 120 per minute. It appeared that the disease was soon restricted to the knee, which suffered most severely. On June 5th the temperature was 104° and the pulse 130. By June 29th the disease had somewhat subsided. The pain was terrible, and, although blisters always gave some relief, morphia injections were necessary.

When I saw Mr. D—— the gonorrhœa was quite well; but the right knee was quite stiff. He was quite free from rheumatism in the other joints, and the knee itself was painless. Mr. D—— had previously suffered from malaria; and a grandfather and aunt were subject to gout.

CASE IV.—*Gonorrhœal Rheumatism—No Arthritic history in family—Symptoms in patient much like Gout.*

Mr. B——, aged 26, a dark-haired man with blue eyes, came to me in April, 1880. He was suffering from his third attack of gonorrhœa, which had lasted three months, and he also had some rheumatic pains. There was no admitted history of arthritic complaints in the family; but Mr. B——, even before his first attack of gonorrhœa, had had rheumatism in the shoulders and elsewhere. Three years before he had slept in a damp bed, and since that time had been constantly troubled with rheumatism, each attack lasting for a few days. At the date of his last gonorrhœa he had little or no rheumatism; but after it he had first pains under the tendo Achillis, next in the ball of the great toe, and then in both knees. In the beginning of February he had gone to Bath, and had been there under treatment and confined to bed for a month. While there he bathed and drank the water. He improved somewhat, and was able to travel to Belfast; but a yellow urethral discharge still persisted. His attacks of gonorrhœa had always been difficult to cure. He had had a bad pain in the right loin, but not much in the spine generally, and in consequence was emaciated and feeble. He had been

accustomed to take wine and beer, and had been told that he might take all sorts of wine except sparkling wines, but had found that port and Burgundy had made him rather worse.

CASE V.—*Gonorrhœal Rheumatism and Gout.*

I first saw Mr. F—— at the end of June, 1892, while suffering from an attack of gonorrhœal rheumatism. He was a young man, aged 22, with an hereditary tendency to gout. His parents were both dead. His grandfather suffered from true gout.

On November 19, 1892, I again saw him. His joints had quite recovered, and he could walk well. Between June and November he had gained nearly two stones in weight. His bowels were constipated. He told me that he thought of going abroad for the winter.

CASE VI.—*Urethritis and profuse discharge in association with Gout.*

(Mr. B——'s case, August 16, 1873.)

CASE VII.—*Severe Gonorrhœal Rheumatism in a robust man of 47—No known family history of gout—Sudden attacks of extremely painful arthritis in many joints—Right wrist and many of the digital joints stiffened—Patient himself probably gouty.*

(Mr. R——, August 31, 1887.)

CASE VIII.—*Spontaneous Gonorrhœa in a married man who denied exposure to risk—Cure followed immediately by acute and severe synovitis, first of one knee and then of the other—Great effusion and much pain—Cardiac Bruit (? old).*

CASE IX.—*Severe Gonorrhœal Rheumatism in a man aged 34, whose father had been crippled by Rheumatic Gout —Hydrops Articuli of both knee joints.*

CASE X.—*A man, aged 34, whose father was crippled with Rheumatism—A fourth Gonorrhœa, followed by Rheumatism with much pain—Knees chiefly affected—Subsequently chronic Hydrops Articuli of both knees—No known history of true Gout. (Daybook, November 8, 1893.)*

CASE XI.—*Gonorrhœal Rheumatism in a young man, who inherited Gout.*

"My father has gout, and all my relatives."

CASE XII.—*Gonorrhœal Rheumatism with Gout history in a young man aged* 21.

(May 3, 1892.)

CASE XIII.—*Gonorrhœal Rheumatism with inheritance of gout.*

Whilst this has been going through the press, I have seen a gentleman who told me that he had formerly had a bad attack of gonorrhœal rheumatism. "Do you inherit gout!" I asked. "No, but I expect I shall, for my grandparents suffered from it," was his reply.

TERATOLOGY.

No. I.—*Intra-Uterine Amputations and Amniotic Constrictions.*

Two very interesting examples of deformities of limbs resulting from amniotic constrictions (the so-called intra-uterine amputations) have come before us recently at the Clinical Demonstrations.

In the first of these, a little boy of eight had lost all the fingers of his left hand. The amputations had taken place at the knuckle joint, and no bone remained lower than the head of the metacarpal bone, with the exception of the little finger, in the stump of which a small portion of the first phalanx could be found. In all a small nipple-like portion of skin, with a minute nail (the extremity of the digit), remained attached by a constricted neck. The thumb was as large as that of the other hand, and the hand itself, with the exception of its digits, was well formed, and had only in a slight degree failed in relative growth. The boy had no other defects, and there had been none known in his family.

In the second case the deformations were much more severe. In both lower extremities a constriction had existed in the mid-leg. On the right side only a deep groove had been left, the distal portion of the limb having maintained its growth in spite of the temporary strangulation. In the left limb great failure of growth had resulted, and a small deformed foot, with webbed toes, hung uselessly attached to a stump a little below the knee. There were defects also in the left hand, most of its fingers being dwarfed.

In demonstrating these patients, I took occasion to remark how conclusively the conditions proved that the cause of the deformity had been local and of a mechanical kind. The limbs were perfect in their development down to the point of amputation or constriction, and not in the least atrophied. There was no indication of localisation by nerves or blood vessels, and no compensatory hypertrophy of any other parts. Although the extremities are usually the parts involved, yet the defects produced are not strictly acroteric. On the contrary, the very tips of the digits may survive when the intervening parts are absent. In almost all these amputations, even when an entire limb is involved, the scar is not complete, but is interrupted in its middle by a small pedunculated nipple, which often, by the presence of nails, may be recognised as representing the very extremity of the member. This makes it tolerably clear that the arrest is not due to arterial obstruction. Probably the constriction is constituted at an early period of fœtal development, and before the vascular system is laid down. In a large majority of cases no real amputation takes place, but only an arrest of growth, which arrest affects most severely the parts nearest to the seat of the constriction. This was remarkably illustrated in the case in which the foot remained, whilst the leg was almost wholly wanting. In some cases, however, the "amputation" is complete, but even in these it must be clearly remembered that it is not a formed limb which is cut off, but only the embryonic tissues from which a limb would have developed.

No. II.—*On Peculiar Conditions in the formation of the Lower Jaw—the "Hippo-notch."*

The lower jaw of the Hippopotamus is peculiar in having a strongly projecting angle. In front of this angle there is a wide and deep notch, so that if the jaw be placed on a table several fingers could be passed under its body just in front of the angle. In this animal the condition referred to

is no doubt due to excess of development of the angle, and the purpose is probably leverage for the action of the masseter. I mention it here because I wish to describe a condition often met with in the human subject, which I have been accustomed to recognise under the convenient name of the Hippo-jaw. A well formed human jaw, placed on a flat surface, will touch it at all parts from angle to symphisis. In the condition referred to, however, there is an arch, more or less marked, just in front of the angle. It is not, however, I think, the result of large development of the angle, but of arrest of that of the body, and it is generally indicative of rather feeble organisation than otherwise. At any rate the lower jaw as a whole is almost always small. A large well-formed chin is most usually accompanied by a level border to the body of the jaw. The notch described is more frequently met with in women than in men. I have not observed it in association with rickets, but often with short stature, slender frame, and other indications of somewhat defective general development.

The lower jaw is perhaps more than another single bone in the body liable to arrest in growth. We not unfrequently see persons in whom it is so small as to almost constitute a deformity. The chin recedes almost into the throat. I have photographs of one or two such subjects, but I do not know of any museum specimen. Such jaws always, I think, have the hippo-notch, and almost always accompany feeble organisation.

No. III. — *Congenital defects in the development of the extremities in several members of the same family—Absence of the Radius, &c.*

A family, two of whom were sent to me by my friend, Dr. Stocker, of Forest Gate, furnished a most interesting example of defective development of the extremities as regards their bones and muscles. The mother of the children had been twice married, and by her second husband had six children, of whom four had their extremi

formed. Of these, two were dead and two were brought to me. I was told that all four had been affected in a very similar manner, though in different degrees. One of the most conspicuous defects in all of them was the entire absence of the radius. Both father and mother were well developed, and neither of them knew of any history of malformations in their predecessors. Of the two children brought to me one was a girl of five, short for her age, but able to walk well, and displaying no definite malformation in her lower extremities. In her upper extremities the radius was absent in both, but the hands were perfect. The forearm was very short, the ulna somewhat bent, and the hand pushed over to the radial side. The upper arms were exceedingly flabby. On account of the child's age it was very difficult to ascertain what muscles were absent, but I thought that the deltoids certainly were so, for although she could lift the extremities she did it by fixing the humeri and moving the scapulæ. I could not definitely feel either biceps or triceps, but the child enjoyed so much use of the limbs that it was difficult to believe they were wholly absent. In the younger child, also a girl, an infant of ten weeks old, the defects were much greater. The radius in each forearm was wholly absent, and the bent ulna only an inch and a half long. All the digits were present, but the thumb in the left hand was tied down towards the palm by a band, and in the right hand was reduced to a mere stump without any nail. Some of the other digits of this hand were contracted and somewhat webbed. The lower extremities were habitually bent almost into the position in which a tailor sits. The knees were flexed and the legs crossed. The legs were very flabby, more especially the thighs, the quadriceps extensor being almost certainly absent on both sides. The hamstring tendons, on the other hand, could be easily felt. The feet were in the position of talipes calcaneus. I thought that there was a small patella on each side fixed to the condyles of the femur. It was impossible to feel sure whether or not the shaft of the fibula was present.

No. IV.—*Anterior Dichotomy in a Bird—A two-headed Finch.*

In the Museum of Antiquities at the Hague there is a beautiful coloured drawing of a two-headed finch (bullspink). It was painted in 1747, and the bird is described as having been found in a garden (probably killed). I could obtain no verification of the drawing—neither stuffed specimen or skeleton—but the condition is credible. The bird is represented as a well-grown adult in full plumage. It has two legs only, which are wide apart, and its tail is single, and it has but one pair of wings. It has, however, two perfect heads side by side of equal size and well shouldered. One head and neck shows the plumage of a cock and the other that of a hen.

This latter circumstance must be allowed to throw some doubt upon the authenticity of the representation.

No. V.—*Albinism in Birds.*

In the Amsterdam Zoological Gardens I saw on October 1, 1894, a beautiful white jackdaw, "Corvus monedula var. alba" (living). Every feather was white, as also its beak and claws. It was not, however, a complete albino, for its pupils were black and its irides a light grey (the normal colour).

I much doubt whether birds are ever full albinoes. I have never seen one, and although some stuffed specimens have red eyes (glass ones), the evidence as to their condition during life is seldom trustworthy.

THE NERVOUS SYSTEM.

No. LXXVII.—*A peculiar form of Symmetrical Morphœa attended by ulceration.*

In Mrs. K——'s case the diagnosis lay between serpiginous syphilitic ulcerations and morphœa with ulceration. She had been treated on the syphilitic hypothesis up to the time of my seeing her, and as such the case was sent to me. I was told, however, that there was nothing in the family history to corroborate the suspicion, and that Mrs. K—— was the mother of seven healthy children. Her husband, now dead, had been a strong, healthy man. He was forty years her senior. She was a lady of about fifty. When I first saw Mrs. K——'s knees I made no doubt that the disease was specific. On the sides of both knees were what looked like large pale scars, and upon these scars many small ulcers. I felt no doubt that it was an example of ulcerated gummata. The symmetry of the affection, however, puzzled me, and also the fact that on both sides the conditions were exactly alike, and that the extent involved was very large, whilst there was but little thickening. Having obtained a better exposure of the limbs and made a more careful inspection, it became clear that what looked like scars were really large patches of scleroderma (morphœa) in the ivory or lardaceous stage, and attended by ulceration. The patches extended above and below each knee on the inner side over areas as large as the outspread hand. They stiffened the knees and made walking difficult. This diagnosis fitted with the patient's history, for the patches had developed spontaneously about a year before I saw the patient, and, as already stated, the history of syphilis was wholly absent.

The occurrence of ulceration in association with morphœa has been frequently observed. It may even be complicated with gangrene. The sclerodermic condition of the ivory patch is one of reduced vascular supply, and may easily, if the patient be feeble or the patch very large, induce molecular sphacelus. An ulcer of this causation was shown in the portrait which I published last summer in the *British Medical Journal.*

No. LXXVIII.—*Epilepsy after Syphilis.*

The following is the synopsis, with schedule, of a case in which most satisfactory recovery has followed ominous symptoms.

Severe Tongue-biting Epilepsy in the second and third years after Syphilis—Subsequent liability to headaches and torpor—Apathy and taciturnity—Lupoid patches on skin—Long-continued specific treatment—Complete recovery—Report extending over nine years.

	YEAR.	AGE.	DETAILS.
1	1888	33	December. Came to me. Had had syphilis in May. Eruption still persisting.
2	1889	34	October. Herpes on right side of pharynx. Arsenic and iodide. Tongue-biting epilepsy in November.
3	1890	35	Liable to headaches on waking; sleeps heavily. Another fit, and a third.
4	1891	36	Returned to India. Advised to continue mercury and iodides.
5	1892	37	In India. Well.
6	1893	38	In India. Well.
7	1894	39	Wrote to me from India that he was quite well. I advised marriage.
8	1895	40	In India. Well in health. Gumma on shoulder.
9	1896	41	Sudden attack of Menière's symptoms in one ear.

In this case the early treatment was irregular and inefficient. When Mr. S—— came to me in December he still had some eruption remaining, although his chancre had been in May.

The three fits were sudden and severe. They were followed by a period of liability to headaches, somnolence, and apathy. Some syphilitic lupoid patches were at the time present on the skin.

In January, 1896, Mr. S. reported himself in excellent health. He had been for five years free from fits.

When Mr. S. came to me in June, 1896, I found that he had several depressions in the surface of his skull where nodes had formerly been. He had also a scar on his left shoulder, adherent to bone, and evidently the sequel of a gumma. These lesions had occurred within the last year, and had been cured by the iodides. He was in good general health, and quite free from brain symptoms. He had recently, however, suffered an attack of acute Menière's symptoms, and his left ear had remained ever since very deaf. With it he could not hear a watch when pressed on the meatus. His account of the attack was interesting. One day in October he suddenly felt his left ear uncomfortable, as if stuffed. He put some warm glycerine into it, but the discomfort increased, and he became so giddy that he could not stand. There was no vomiting. He went to bed, and when recumbent was quite comfortable; but for forty-eight hours he was unable to raise his head from the pillow without experiencing distressing giddiness. When the attack passed off he felt very uncertain in his legs, and was almost absolutely deaf in the affected ear. It was a month before he could play lawn tennis, and there had remained a constant buzzing in the ear. No giddiness had recurred.

No. LXXIX.—*A case illustrating the importance of pain as a symptom of defective blood supply.*

In several cases which have been recently published in ARCHIVES I have endeavoured to draw attention to the symptom of pain as caused by defective supply of arterial blood. Its occurrence and severity in senile gangrene has long been recognised, but as a matter of fact it is often present in greater or less degree long before any gangrenous

process is set up. Sometimes it is only a general aching or sense of excessive fatigue, but at others it amounts to really severe pain. It is always much influenced by posture; many old persons suffer more or less from it, more especially those in whom the arteries are becoming calcareous. Some of the cases which I have cited were, however, not in old persons, but in those of middle age in whom the arteries had become occluded. I have just seen a senile case which seems worth narrating.

Mrs. B—— is a lady of 72, who is supposed to have taken stimulants rather freely, but who still enjoys good general health. For three years past she has suffered much from aching and gnawing pain in both legs. The pain would shift about from part to part, and has been so severe that she has frequently sought medical advice for its relief. It has been always aggravated by the erect posture, and for more than a year Mrs. B—— has abstained from walking and almost from sitting. She has even attended church in a recumbent bath-chair. In bed until quite recently her limbs have usually been comfortable, but the moment she has put them to the ground they have begun to ache. Yet during this time there has been no lesion of nutrition in the feet. All that has been observed has been that slightly dusky patches would sometimes form, and now and then spots almost like purpura (thrombotic). It has been recognised all along that the radial arteries were large and ribbed with calcareous deposit.

I was asked to see Mrs. B—— because gangrene had attacked several of the toes of her right foot; this had been attended by great increase of the pain. I made a careful examination of her arterial system. Both radials were large and extensively calcareous, but not to the extent of preventing pulsation being easily felt. The carotids were large and somewhat rigid. The temporals were small and showed no rigidity; that on the left side was found with difficulty. The femorals were large and rigid, and so also were the popliteals. The latter being enlarged had become more superficial than natural, and were very easily felt. In the right leg the anterior tibial could be found a little above the ankle but not lower. On neither side could I f

posterior tibial, and on the right (the gangrenous foot) I could not find the anterior tibial at all. Both feet were pale, and showed pinkish or slightly dusky patches. The gangrene had begun simultaneously on three different toes, and was in the main of the dry form. I inquired particularly as to the character of the pain which had been present, and was told that it is sometimes aching, sometimes shooting, and often of a gnawing kind. Often after an hour's sound sleep it would be exceptionally severe. It is to be added that some months before my visit and before the gangrene, Mrs. B—— had been kept in bed continuously for six weeks, in the hope of relieving the pain in her feet, and, as it was thought, with definite benefit.

No. LXXX.—*Recovery from Spinal Paralysis after concussion.*

About four years ago I went over to High Barnet to see a farmer who was paralysed. I have mislaid my notes, but I remember that he could not move his lower extremities at all, and the upper ones but very little. The history was that about six weeks before I saw him he had fallen backwards out of a cart. His paralysis did not follow immediately on the injury, for he had been able to follow his occupation for some days. After that he had made two journeys to London to consult a nerve specialist, finding his legs becoming weak. Gradually the paraplegia had increased until it became complete.

I saw this case with Dr. Thyne. Our treatment consisted in the use of mercury. I have just learned from Dr. Thyne that the patient recovered perfectly, and that he is now quite well, and able to exert himself almost as well as ever.

No. LXXXI.—*Very frequent recurrence of Herpes preputialis—Cure by a long course of arsenic.*

A young gentleman, named R——, who consulted me in November, 1886, was liable to the occurrence of herpes on

his penis so frequently that he was scarcely ever free from it. It always showed a tendency to get well, but as soon as the parts were healed a fresh crop of vesicles appeared. He had had a chancre two years previously, but no secondary symptoms had been recognised. He said that the first herpes had occurred after the use of a catheter. Under treatment—arsenic with mercury—in January, 1887, my notes state that herpes occurred only once a month and was milder. Subsequently under continued treatment he became free from it.

No. LXXXII.—*Exceptional forms of Herpes Zoster.*

August 27, 1895.—I have seen this morning two somewhat rare locations of herpes zoster. In one the region of the lesser sciatic was involved, and in the other that of the genito-crural. In the first the eruption covered the buttock and back of thigh, the lowest group being in the popliteal space. As usual, the largest and best developed patch was in the middle just below the fold of the buttock. The lowest and some of the upper ones were almost abortive, being denoted by redness only and having no vesicles.

In the second case the symptom of warning had been pain in the testis, after a day or two of which large groups of vesicles had developed all over the front and inner side of upper part of the thigh. When the vesicular stage was reached the aching of the testis came to an end. Both these cases began during a period of much electrical disturbance, violent thunderstorms, &c.

A third case of herpes zoster which I have recently seen is also worthy of note, inasmuch as the eruption came out of the digits to their very tips and in the palm of the hand. It is very seldom that herpes zoster affects either the hand or the foot.

No. LXXXIII.—*Herpes Zoster affecting the upper extremity and extending to the palm of hand and tips of fingers.*

Such exceptional cases are, I think, usually met

old age. I have during the last year seen two in which the hand was involved, and both were in old men. One of these, which occurred to a member of our own profession who has reached the age of 83, is worthy of a brief description of the precise location of the eruption. There were dorsal patches in a line with the spine of the scapula, from these others passed along the outer border of the axilla; other larger ones occurred on the back of the upper arm, and from this region winding a little forward on the back and radial border of the forearm. Finally the palm of the hand was involved, and vesicles occurred to the very tips of the fingers, exempting, however, the little finger and the thumb. There was no eruption on the back of the hand.

When I saw Dr. W—— it was several weeks after the outbreak, and only the scars remained. These were, however, very conspicuous. The limb was somewhat wasted, but perhaps not more than the compulsory disuse would explain. There had been very severe neuralgia in the limb. It was still impossible to let the arm hang down on account of the aching which immediately resulted. The veins would at once fill and the hand begin to swell.

No. LXXXIV.—*Death from Herpes Zoster.*

I attended not long ago a lady, of near 70, in whom a severe attack of herpes affecting the cervical plexus (neck and clavicular region) ended fatally. She had, however, for long before been in feeble health, and suffered from heart disease. The herpes caused great pain, and produced such an increase of debility from sleeplessness, &c., that she sank in the fourth week.

MISCELLANEOUS.

No. CCXXVI.—*Gonorrhœal Rheumatism with history of Hereditary Gout.*

I saw with Mr. Reginald Jowers, at Brighton, on November 19, 1892, a very severe case of gonorrheal rheumatism in a young man of twenty-two. He was at the time of our consultation confined to bed, and his knee joints were much swollen. I am induced now to mention his case, because it confirms the opinion which I have long entertained, and often expressed, that an inheritance of gout gives proclivity to gonorrheal rheumatism. There was a strong inheritance of gout in this young gentleman's family, and especially one of his grandfathers had suffered severely. As regards treatment, I may add that, under liberal counter-irritation of the joints with quinine and aconite internally, he made a good recovery, and when he called on me, five months later, in town he was able to walk well, and had gained two stones in weight. His right foot, the instep of which had been affected, had become somewhat flat.

No. CCXXVII.—*Numbness of Skin caused by Iodide of Potassium.*

"I see that when you consulted me three years ago you complained that your legs were becoming numb, and that you thought you were going to lose the use of them." "Yes," replied my patient; "but what you said proved to be quite right, it was due to the quantities of iodide of potash which I had taken. I left it off after I had seen you, and soon got rid of the numbness, and have never had it since." This case is only one among many whic

convinced me that the iodide often produces symptoms which simulate sensory paralysis and cause much anxiety to the patient.

No. CCXXVIII.—*Case illustrating the treatment of Urticaria.*

I have during the last six months had under observation a lady who has suffered long from a peculiar form of tuberose urticaria. Her attacks would often affect the face and cause much swelling, and sometimes the throat, causing so distressing a sense of suffocation that she feared she would be choked. I mention the case now without giving details, in order to record the fact that under a four months' use of a mixture, containing in each dose two grains of quinine and a drachm of tincture of bark, the liability has quite ceased and the general health much improved. We had previously used arsenic without any obvious benefit. It is of interest to note that at the end of the quinine treatment an attack of shingles on the side of the chest occurred.

No. CCXXIX.—*Pustular Ophthalmia in association with "Bazin's legs."*

The association of pustular ophthalmia with Bazin's ulcers on the legs has, I believe, been repeatedly noticed. It is a fact in favour of the tuberculous nature of this form of eye disease. I have just seen a lady whose case has been fully recorded as a typical example of Bazin's malady, and who has often had inflamed eyes. Although now apparently in good health, florid and robust looking, she has recently had a relapse, in which her eyes inflamed and ulcers broke out again on one leg. The form assumed by the ophthalmia was that of painful little phyctenulæ close to the corneal margin, from which superficial ulceration extended a little way upon the latter. It was a type-form of "pustular" or "phyctenular" ophthalmia. It has yielded to Pagenstecher's ointment pretty readily, as indeed it had done on former occasions. This patient has been definitely threatened with phthisis, and has large scars of strumous abscesses in her

neck (from childhood). Her legs are covered with scars. She is now nearly fifty, and has had one breast removed for scirrhus. Are we, in such a case, to suppose that tubercle bacilli remain in a dormant condition between the various attacks of strumous inflammation from which she has suffered; or is it to be held that the latter have been due to tissue-proclivity independent of the parasite? She has suffered on and off from the ulcers on the legs for nearly twenty years.

No. CCXXX.—*Multiple Subcutaneous Swellings in a young child, probably tuberculous—Subsequent disease of Elbow Joint.*

About a year ago I saw, in consultation with Mr. Gunn, at a house in Hampstead, a little boy of about eighteen months of age, in whom it was thought there was a suspicion as to inherited syphilis. His left eye was inflamed and lost. I do not now recollect its exact condition. The point for which I now mention the case is that the child had a number of little subcutaneous swellings, seven or eight in number, at different parts of his body and limbs. I thought that these were most of them threatening suppuration, and were probably of the nature of abscesses from infection. It was advised that the eye should be excised, and I strongly urged that at the same time these abscesses should be incised and carefully scraped out. It was agreed that the boy should be sent for a long residence at the seaside. I could not detect any evidences of syphilis, and did not think that the tumours were new growths.

I did not see anything of this child again until a year later. To-day, April 16, 1895, he has been brought to me on account of stiffness of the left elbow. He is now a fine and well-grown boy, apparently in excellent health. The abscesses, which were scraped out, have left quite sound scars, and no new ones have appeared. The affection of the elbow joint is, I have no doubt, strumous, and probably beginning in the bone. There is no material swelling, but the joint is becoming stiff. The child had resided durin

the whole year at Margate with every possible advantage. I advised that the elbow should be put into a leather splint.

No. CCXXXI.—*Cancer of the Tongue in a very early stage—Free Excision—Development in the glands within six months.*

A feature of interest in the following case is that the ulcer at the time of its excision was exceedingly small. It was not larger than a threepenny-bit, and presented features by no means likely at first sight to cause suspicion. Yet, as the result has proved, it had already caused infection of the lymphatic glands, for a large growth rapidly followed in them soon after its removal. No case could more strongly emphasise the importance of the doctrine of the pre-cancerous stage and of very early operations. Mr. A——, a gentleman aged 45, was sent to me by Dr. George Birch, of Clapton, on June 12, 1895. He had been aware of slight soreness of one side of his tongue for nearly a year. The sore, which was very small, was just under an upper molar which was stopped with amalgam. As the sore, although small and quiet, presented a granulated surface and a slightly hardened base, I had no difficulty in advising its immediate removal. This I did in my consulting-room, by cutting it freely away by means of curved scissors. The report sent me by the Clinical Research Association was: "The section shows a superficially ulcerated squamous-celled epithelioma. The down-growths have invaded the muscular substance. There are a few cell-nests."

I did not see Mr. A—— more than once after the excision. He was an exceedingly nervous man, and dreaded further operations. So when his glands began to enlarge he did not come to me. I heard almost accidentally about six months after the excision that he had large gland-masses under his jaw and was not likely to live long. In proof that the removal of the primary disease was adequate, there had been no return whatever in the scar.

No. CCXXXII.—*The congenital form of Dupuytren's contraction of fascia.*

March 24, 1896. I have seen two cases to-day of the congenital form of Dupuytren's contraction. In one of them a surgeon had it in only one of his little fingers, or at any rate the tendency in the other was so slight that I could not be certain of it. He told me that his father had it, but that out of a family of eight, he himself was the only one who had inherited it.

In the other case both little fingers were affected. The patient, a middle-aged man, did not know that any predecessor had had it.

In this congenital form, the little finger is usually the only one affected; though there may be a suspicion of thickening in the palm, it never amounts to contraction. The location of the band which bends the little finger is never in the palm, but just in front of the first phalangeal joint, which it contracts at an obtuse angle like the string of a bow. The best, and I believe the first account of these congenital cases, is by Mr. William Adams. They are often hereditary and seldom aggressive.*

No. CCXXXIII.—*Psoriasis with local Blanching of the Hair of the Moustache.*

Mr. B——, jun., who is the subject of psoriasis, has a one-sided white patch of hair in his moustache. He says that it has been there ever since the hair began to grow and that he knows of no cause. It is quite stationary. It comes close to the middle line on the right side. It is a question of much interest as to whether there is any connection between it and the skin disease.

* See also a mention of the same subject at page 291.

NOTES OF DEMONSTRATIONS AT THE CLINICAL MUSEUM.

ONE of our Museum portraits, representing *Keloid of Scar*, was taken from the arm of a young lady in whom the scars of vaccination had been attacked. The vaccination was done at the age of nine, and the portrait (by Miss Green) was made five years ago. The patient is now thirty-four years of age, and she was good enough to attend at a recent Demonstration to allow her arm to be inspected, and its present state compared with the conditions shown in the portrait. I was able to prove by this comparison that not only had there been no extension of the keloid patches, but that these were much less elevated and less glossy than formerly. They were becoming soft and evidently in process of atrophy. That they will ultimately disappear may be confidently foretold.

This case is of much interest on account of the extensive growth of the keloid. It had not remained as rounded buttons occupying only the vaccination scars, but had prolonged itself in tongues of at least six inches in length. Three satellites had also been produced over the scapula, at a distance of several inches from the parent patches, there being none on other parts of the body. I took occasion to remark on the latter fact that it proved that the keloid process was one attended, like almost all others of inflammation or new growth, by the production of infective elements capable of transference through lymphatic spaces, and able to originate similar growths at a distance. In most instances these elements travel only a short way, and produce satellites near to the parent patch. In a few very rare examples, and especially if the patient have old scars, these may be attacked at such a distance as to suggest that the germs had found con-

veyance by the blood. In the present I remarked that it was not improbable that little scars of boils or acne had preceded the keloid buttons on the shoulder and offered a nidus.

Amongst the most interesting cases which have come before us have been some which illustrated the *Accidents of Vaccination.* Of severely pruriginous eruptions we have had several, and of extensively generalised vaccinia one of the most remarkable that I have ever seen. The facts of this latter, a fatal case, have been fully recorded in ARCHIVES, Vol. VII., p. 43, and I need not recapitulate them. It might perhaps be convenient that this form of vaccinia, tending as it does to end fatally and to present very perplexing phenomena, should be known as Vaccinia Maligna. It is at any rate very important to recognise that there is a malignant and occasionally fatal form of vaccinia. Under this head I should place the now celebrated Leeds case and others resembling it. The terms "varioloid vaccinia" and "vaccinia gangrenosa" (which I have previously used) are in strictness applicable only to certain examples of this group.

A few weeks ago Dr. Knowsley Sibley was good enough to bring us an infant showing a most remarkable vaccination eruption. The case was of emphatic importance because it might so easily and pardonably have been mistaken for syphilis. The infant was covered with a dusky erythematous and papular eruption, which on the buttocks and thighs had ulcerated and presented superficial sores exactly like those so common in congenital syphilis. The face was of an earthy hue, and a copper-tinted erythema showed a preference for the corners of the mouth. The "copper" or "lean-of-ham" colour of the eruption was everywhere most characteristic. If the child's trunk and buttocks had been sketched, the portrait might have served well as a typical representation of a specific eruption. Yet in spite of these appearances I do not think that there was any syphilis in the case, nor do I believe that Dr. Sibley nor any of those present at the investigation of the case differed as to diagnosis. The infant was fat and well-grown. She had no snuffles, and not the slightest trace of congestion of mouth, throat, or nostrils

The eruption had commenced on the eighth day after vaccination near to the pocks, and had become generalised within a fortnight. The pocks themselves had gone on favourably all the time, and were now healed without the slightest induration. If the case were syphilis it was from congenital taint and certainly not from vaccination. This the progress of the pocks, the early advent of the rash, and the whole appearance of the latter made obvious. Now as to congenital taint, the mother, who brought the infant, appeared to be quite well. She reported her husband as being well, and a little boy who accompanied her appeared to be so also. The patient was the youngest of four, and all the three elder were living and well. It is further somewhat improbable that a tainted infant should remain quite free from symptoms until vaccinated, and then within a fortnight develope an eruption covering the whole surface.*

Much of the infant's eruption appeared to be almost urticarious in character, though the wheals were not perceptibly raised, and were so numerous that they covered almost the entire surface. The child did not appear to suffer much, and still took food well. It was on the twenty-third day after vaccination that it attended for demonstration.

Nearly a year ago I produced at one of the Demonstrations a young man who had that morning been brought to my consulting room. His complaint was that he could not rise from his chair without pain in his belly, and that it was uncomfortable to stand upright. He had kept at his occupation (in a chemist's shop), and did not consider himself seriously ill. I found on stripping him that he had a large rounded tumour in the lower part of his abdomen. It was very easily felt, and could even be seen, for he was thin and it occupied almost the whole space from the umbilicus to the pubes. It bulged more to the left than to the right. It was quite painless excepting under the conditions mentioned, and at the demonstration he allowed all to examine

* It may be noted that the palms and soles and the flexures were free from eruption, and that, although the face was dusky, it did not show actual spots.

it freely. It was thought that there was an obscure sense of fluctuation, and the diagnosis which found most favour was that of hydatid tumour. It was impossible, however, to feel any great confidence in this opinion, but as it was clear that something should be attempted, I advised him to go into the London Hospital under the care of my son. A week later my son reported to me that he had done an exploratory operation, and that the diagnosis of hydatid had unfortunately not been confirmed. An incision had been made in the middle line from a little below the umbilicus to near the pubes, and an extensive and thick mass of crude tubercular deposit had been cut through. The peritoneal cavity had been opened, but the chief thickness of the deposit was found between the peritoneum and the transversalis fascia. It was so extensive that it was judged to be quite impracticable to effect any useful purpose by scraping or cutting it away, and accordingly after removal of some fragments the wound was closed. Careful antiseptics had of course been used, and the wound afterwards did well. In the course of a few weeks the youth was able to get out into the garden. The wound had not closed in the middle, but the amount of discharge was very moderate.

I have just seen the subject of this case again. It is now nine months since the exploration. Contrary to what was feared and expected, recovery has taken place. The young man has been six months in the country, is now in his usual health, and is about to resume his occupation. He has no discomfort in the abdomen, and can walk freely. There is still a sinus leading about three inches into the middle of an induration not bigger than a duck's egg. The surrounding parts are wholly free from induration, and quite soft.

Many remarkable cases of recovery after laparotomy for ascites with tubercular conditions of the peritoneum have been known, but this case differs, I think, from all of them. There was no fluid in the peritoneal cavity, and although some tubercular formations were present on the surface of the serous membrane, yet the great thickness appeared to be extra-peritoneal. The portions removed were, in relati

to the whole, insignificant. When the operation was concluded, no hope was felt as to any real recovery. The case is of importance not only as to diagnosis, but as an encouragement to operative treatment. We must regard the cure as having taken place under a process of aseptic softening, and finally of suppuration, for antiseptic dressings have been long abandoned.

A very interesting example of patchy *Pigmentation of the Lips and Mouth* was brought for our inspection by Dr. Dixon on June 23rd. Its subject was a dark-complexioned married woman of about forty. Her lips, both upper and lower, showed on the prolabia blotches of bluish-black pigment, most of them about the size of split peas, but not accurately rounded. Others were seen inside the lips, in the cheek pouches, and very conspicuously on the hard and soft palate. There were none on the gums or tongue. The woman was not obviously out of health, and had no indications of Addison's disease. She was a dark brunette, and said that in youth she had been much darker, and very prone to tan on exposure. The spots on her lips had been first observed about three years ago, and were increasing. They caused her no inconvenience. Simultaneously with their appearance a small coal-black spot not bigger than a pea had made its appearance on her forehead over the left eyebrow. It might have been supposed to be melanotic sarcoma were it not for its very small size and the absence of thickening.

In illustration of this case I produced some portraits showing senile, infective freckles on the face, and mentioned a case which I had seen on the same morning in which black patches had formed on the lips of an elderly clergyman who had been a great smoker. The portrait of Dr. Conner's twins was also brought forward. In these latter I was able to state, on Dr. Conner's authority, that since the portrait was taken, now two years ago, there had been no obvious increase in the pigmentation. The chief feature of similarity to our present patient was in the location of pigment patches and in the very dark complexions of the

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subjects of them. Clearly it may be suggested that the amount of pigment congenitally present has something to say to the production of these pathological patches.

The portrait which, however, seemed most closely to bear upon our case, is one which was copied by Mr. Willett's permission from one taken in St. Bartholomew's Hospital, showing the mouth of a woman under his care. In it pigment patches on the lips, cheeks, and gums had been aggressive for many years, and had finally been accompanied by the production of a sarcomatous growth, not pigmented, in the upper gum. This development of sarcoma in association with infective pigmentation is exactly what I have observed in several of the cases which I have recorded under the name of "senile freckles."

Dr. Fletcher, of Camden Road, brought us on June 9th an interesting example of the form of *Dupuytren's contraction which occurs in Children.* The patient was a girl of about twelve. As usual in these cases the little fingers were alone affected, and the band was in front of the first phalanx and metacarpo-phalangeal joint, rather than, as in the adult form, in the palm itself. It was attributed to "rheumatism," and was stated to have developed within the last three or four years. It was, however, quite certain that the joints were not stiffened, and that the deformity was caused by a broad band which, like the string of a bow, held the second phalanx in a bent position. The Museum possesses a valuable series of casts illustrating this curious affection, the gift of Mr. William Adams. The evidence which I have obtained suggests, as in this case, that it occurs in association with the inheritance of rheumatism and gout. Mr. Adams, however, doubts this.

Dr. John Dixon, of the Temperance Hospital, sent us a very exceptional illustration of *Gland Disease.* The patient was a German Jew. In Scarpa's triangle in each thigh there was a large mass of very hard glands. The glands were adherent to some extent, but were not inflamed, and showed no tendency to suppurate. They were exactly in the pos-

tion of what is known as Hebra's Bubo, a form of gland disease which occurs in association with pruriginous eruptions on the legs. In the present instance, however, only the most trivial patches of eczema could be found on one leg. Nothing whatever which could have been supposed adequate to explain gland irritation could be seen anywhere. The glands were hard enough for sarcoma, but then not only could no primary disease be discovered, but the condition was symmetrical. A parallel case to this had been several times under our observation some months before. In it the patient, a comedian, had large gland masses in both sides of his neck. The glands were very hard, and adhered together, but showed no signs of suppuration. The man was near fifty, and no primary disease could be discovered anywhere.

To Dr. Stocker, of Forest Gate, we were indebted, on June 23rd, for a good example of the *Multiple Fatty Tumour of the Extremities*. The patient was a healthy man of fifty, who stated that his father had had similar growths. There were five or six on each forearm, and one on the front of the right thigh. The last was the largest, being as big as a small fist, whilst the others were not larger than chestnuts. They were all, as usual, of firm structure (fibro-fatty), and adhered loosely to the skin without involving it.

Two examples of *Melanotic Sarcoma* have been recently under our observation. In one a young man had numerous secondary subcutaneous growths in various parts. They had followed a growth in a mole, which had unfortunately been neglected in the first instance. It had been excised when too late, and with it several secondary ones. It was clearly too late to do anything further. A very important fact in the history of the case was supplied by a letter from Mr. Nettleship, which informed me that he had excised one eye from the father of the patient on account of a melanotic growth, and that he, the father, had subsequently died of the disease.

In the second case I could produce only the specimen

after excision, the patient being unwilling to attend. The specimen was a nodule of coal-black growth, not larger than a pea, which had been excised from the temple of a young lady of eighteen. It had not been preceded by any mole. There was a history that one of the girl's uncles, a young man, had had his arm amputated by Sir Benjamin Brodie for fungus hæmatodes, and had died of secondary growths, and that a grandmother had died of cancer.

In connection with these two cases, both of them remarkable examples of heredity of tendency to cancer, I took occasion to discuss the probable nature of such inheritance. Avowing absolute incredulity as to any parasitic cause of malignant action, I remarked that it was very certain that the primary growth could originate germ-elements of an infective character, which could travel through the lymph spaces or in the blood-vessels, and develope in parts more or less distant. Two questions would occur for consideration before we could decide as to the nature of the inheritance. First, how nearly molecular may such elements be in size; and next, how long may they remain living whilst wholly dormant. Both of these might probably be answered with the utmost liberality. The contagious particles may probably be infinitely minute, and thus capable of passing with sperm or germ; and next they are probably capable of remaining latent and dormant, yet still susceptible of development, for indefinite periods of time. I mentioned in support of these theories some cases in which, after the removal of a primary cancer, the patient remained for years without any sign of return, and then developed a growth in close anatomical proximity to the original one. In such instances we can but believe that germinal matter had lain dormant. In favour of the belief that it is not mere tissue proclivity which is inherited but actual particulate elements, I recalled certain cases in which, as in the first of the two just mentioned, the offspring developed precisely the same form of cancer as that from which the parent suffered. In this instance it was pigmented sarcoma (a rare form) in both father and son. In another instance, photographs of which were shown, the father had his face destroyed by rodent

cancer, and his son displayed the same disease at the unprecedentedly early age of fourteen.

Amongst the Drawings which have been added to my collection during the last quarter are the following:—

Symptoms of *Congenital Syphilis actually present at Birth.* The infant was born in the Whitechapel Infirmary, and survived only three hours. Its extremities showed an abundant bullous eruption. Mr. Herbert Larder, the medical superintendent, to whom I was indebted for the opportunity of obtaining the drawing, tells me that he has never seen such marked symptoms present at birth. The mother denied any knowledge of syphilis, but admitted that she had during the last year attended at King's College Hospital for an eruption.

A valuable series of *Photographs of Yaws* has been sent me by Dr. Daniel, from Georgetown, British Guiana. Two of them show the secondary eruption fully out, one patient being a negro and the other a European. They both exhibit a symmetrical eruption affecting the limbs, trunk, and face. The eruption is of a papular and frambræsoid type, and there is certainly nothing in the portraits to enable the observer to distinguish it from syphilis.

CATECHISM OF SURGERY.

No. CCIX.—*Inoculation Experiments on Leprosy.*

Q. Have many experiments been made without success to inoculate leprosy?

A. Yes. A Norway surgeon inoculated himself and twenty other persons. None of them took the disease. Dr. Propeta, in Italy, inoculated himself and sixteen other persons. Nothing followed in any of them.

Q. In the ever-quoted case of Keanu what were the facts?

A. Dr. Arning, with the written consent of the man (a convict), inoculated him. Three years after the inoculation Keanu was a leper. It must be remembered, however, that he lived in the Sandwich Islands, and that his son, his nephew, and a maternal first cousin were all lepers. This case will never be mentioned again as a proof of contagion by any well-informed and candid controversialist.

Q. Have attempts been made to inoculate leprosy in the lower animals?

A. Yes. Melchor and Ostmann thought they succeeded in rabbits. Dr. Kawsin, of Molde, who made many experiments, always failed.

Q. What are the fallacies in the Glasgow cases so frequently quoted as instances of communication of leprosy by vaccination?

A. Two children vaccinated in infancy in Barbadoes from the same vaccinifer became, about the ages of seven or eight, lepers. The vaccinifer had in the meantime developed leprosy. Now all three had been born and bred up in a place where leprosy was endemic. In none did anything unusual occur to the vaccination pocks. The vaccinifer had no leprosy at the time. The vaccinees did not develop leprosy until six or seven years afterwards. Here, I think, are sources enough of fallacy. Probably all three got their leprosy from a food source common to them all.

No. CCX.—*Recapitulation Questions for Self-examination.*

No. 1.—The Urine.

The following questions are given by way of recapitulation, and without any answers. Although some of them are very simple, yet they contain the essentials of urinary diagnosis, and the student who could answer them all straight off might be taken to have a fair knowledge of his subject. Easy as they may appear to be, I speak from experience when I say that some of the easiest have often proved puzzling to candidates for diplomas.

What do urates usually look like?

Under what conditions are they soluble?

What is the most important substance present in urine?

What does a low specific gravity of urine usually imply as regards its constituents?

Under what circumstances may the urine of a healthy person contain albumen?

What chemical change has taken place when urine has become ammoniacal?

Is uric acid soluble by boiling?

What do crystals of uric acid look like to the naked eye?

Does uric acid which is not visible when the urine is voided ever deposit itself when the urine cools?

Do children ever pass free uric acid?

Can you distinguish oxalates by the unassisted eye?

If a man told you that when during micturition he chanced to splash his boots white spots were left on them, what should you suppose to be the matter with him?

What dietetic advice would you give to a patient in whose urine free lithic acid was frequently seen?

Can you distinguish between uric acid and lithic acid?

What is the supposed influence of the consumption of table-salt upon the excretion of free uric acid and the liability to calculus?

How do you explain the frequency of uric acid stones amongst the Hindoos?

In what parts of the British Isles is urinary calculus common and in what rare?

What articles of food are to be avoided in cases in which oxalate of lime appears in the urine?

How would you recognise oxalate of lime under the microscope?

ARCHIVES OF SURGERY.

OCTOBER, 1896.

PRESIDENT'S ADDRESS

AT THE

THIRD INTERNATIONAL CONGRESS OF DERMATOLOGY.

(*Held in London, August*, 1896.)

GENTLEMEN,—I welcome you one and all as members of the third International Congress of Dermatology. Many of you have travelled far to attend it; and to all who are visitors in London, I beg to express on the part of my colleagues our earnest hope that the arrangements which have been made will conduce to an agreeable and profitable sojourn amongst us. I can assure them that no effort towards this end shall be spared, and that our highest ambition is that the London Congress may be in some degree a worthy successor to those of Paris and Vienna.

That it should be possible to assemble, on such a subject as ours, such a Congress as this, is certainly a very noteworthy feature of the times. It is within the memory of some of us that the Dermatologists of Europe might have been counted on the fingers. There were one or two in each of our large capitals, and those were all. Thanks to their labours and to the capabilities of the age in which we live, we now count our colleagues by hundreds. And better still than that, we may now believe that the whole profession takes an interest in Dermatology, and that what was not

very long ago looked down upon as one of the less worthy specialisms, has now taken its place as a branch of medicine of the very foremost value.

If we ask how this most rapid development has come about, the answer must be, "By the aid of cheap printing and cheap travelling." We are now able to communicate easily one with another and to make mutual use of each other's brains. A six hours' journey will now enable a Londoner to visit what I may fairly name the Cradle of Dermatological Science, the Hôpital St. Louis, and to avail himself of the teaching of its professors, the wealth of its wards, and the magnificent collection of models in its unique museum. By the atlases and printed works of Hebra, Kaposi, Neumann and Auspitz the whole world is made almost as familiar with the teaching of the Vienna school as is Austria herself. I must not go further, but it would be easy to mention from other parts of Europe and from the other side of the Atlantic abundance of other instances of mutual indebtedness made possible only by the splendid facilities of modern intercourse. Of these facilities such a Congress as I have the honour to declare open to-day is the final outcome and not one of the least important. That it will not be the last I confidently foretell. Although our organisation is only temporary and ends with the occasion, yet I feel sure that, like the phœnix, it is secure of a renewal of life. The mission of Dermatology is not yet accomplished; rather say it is only begun. Its banners must change hands, but amongst those who press forward into our ranks are many who will not forget the traditions of the past, and by whom they will be upheld with still increasing honour, ability and zeal.

The chief topic of my remarks this morning will be on the value of Dermatology in its relation to general medicine, and to this I will now turn.

The Lessons of Dermatology in relation to Tuberculosis.

We may perhaps find in the topics of Tuberculosis and Cancer our best illustrations of what the study of diseases

of the external integument has been able to do for general pathology.

Koch's discovery of the tubercle bacillus has added much to the precision of our knowledge respecting the diseases formerly classed as scrofulous. The repeated demonstration of this bacillus in lupus vulgaris, in lupus necrogenicus, and other forms of scrofulous sores, justifies us in the claim that these maladies are essentially modifications of a tubercular process. Whilst, however, here gratefully accepting the aid of the bacteriologist, it may be wise to insist that his conclusions must still be subject to the control of clinical observation. More especially does this apply when his conclusions are negative. The failure to discover the microbe in question cannot be taken as proof of its absence, still less can it be allowed to differentiate utterly any two maladies which clinical evidence declares to be alike. Here, as elsewhere, the clinicist and the histologist must work together and be willing to accept help the one from the other. Let us then take it as an accepted fact that the forms of Lupus just named are really tubercular, and see what may be deduced from observations on them as to the general laws of the tubercular process.

First, we have in Lupus Vulgaris Solitarius every day proof that a tubercular process may remain for many years, nay, through a whole life, restricted to one part, and showing no infective powers excepting those of strictly local extension. Cases are abundant in which patients, apparently in good health, have had patches of lupus which for twenty years have slowly advanced at their borders, but have never produced others elsewhere nor been attended by any other manifestations of tuberculosis. It is clear, then, that the infective power of the tubercle bacillus may, under certain conditions, be most remarkably restricted.

Second.—From the facts which have been established respecting *Lupus Vulgaris Multiplex* we learn that even when many patches are present, each single one conforms to the law just referred to, and is incapable of shedding elements which are potent for distant infection. We learn also from these cases the yet more remarkable fact that potency for

distant infection does exist in the first stage. It is this which makes multiplicity possible, and under its influence the skin may be covered all over with spots of lupus. This power lasts, however, for only a short time, and after six months or a year the patches settle down into comparative quiet, and all risk of the production of new ones is at an end.

Third.—The next general law illustrated by Lupus Vulgaris is that a tubercular process which has taken origin in any given tissue keeps for the most part to that tissue, and shows but little tendency to infect others. It might have been thought probable that the tubercle bacillus as a free parasite would, having once gained a footing, have wandered from one structure to another and found its home in various tissues. It might have been expected that we should recognise the duty of destroying a patch of lupus for fear that the bacillus should migrate to the lungs or to the bones. But no; fears of such results never enter into our minds; for although we recognise that lupus vulgaris is scrofulous, we know from experience that it is very improbable that the possessor of a lupus patch will ever show any other scrofulous manifestation. Nor will he even become the subject of any other form of lupus. Lupus vulgaris (lupus of the corium) does not breed lupus erythematosus (lupus of perivascular spaces) nor does lupus sebaceus (lupus of the sebaceous glands). Each of these different forms remains true to the tissue of its birth and keeps its own inalienable type. Do we not from reflection upon these facts arrive pretty certainly at the inference that the bacillus really enters into some sort of intimate partnership with the structures in which it breeds, and that the results which we witness are only in part those of parasiticism, and in yet larger part those of a modified cell-growth? Possibly we may even realise that there may be an affinity between such processes as those of lupus and those of cancer, the one in the main, but not wholly, a disease caused by parasites, the other chiefly due to a morbid mode of cell-production—but, it may be, complicated by parasiticism.

Fourth.—Lupus supplies us with some of the best proofs

that we possess of the possible latency of parasitic elements. Not only may a patient retain through life a lupus-patch which scarcely increases in size, but it may be that local manifestations which have been wholly removed by appropriate treatment in youth and have been absolutely absent through a long life, may again in old age show activity in precisely the part originally affected. I have recently seen two examples in each of which the period of perfect freedom from lupus (of the nose) was thirty years, and yet the senile recurrence was definite and severe. Can such cases be explained in any other way than by supposing that infected cell elements had remained through life in a state of quiescence, but still retained full powers of growth? These cases, I may add, are on all fours with many others which have been observed in other forms of struma, and some of which have given support to the doctrine of "Senile Scrofula" so ably put forth by our veteran colleague, Sir James Paget. Surely these facts as to the long survival of potent, but wholly quiescent, germinal matter are of the utmost importance to the pathologist. Nowhere will he find them better displayed than in diseases of the skin.

The principal facts upon which has been based what I have just said, are, I believe, in general acceptance. Some of those which I shall next adduce are more disputable, and I must claim your indulgence if, under the exigencies of the occasion, I do not stop to offer proof, when, in the opinion of some, it may seem to be needed.

The next pathological lesson which I purpose to deduce from what is observed in connection with lupus, is that from the same parasitic cause very different local changes may result. A certain general parallelism as to conditions of occurrence and course of progress must here assist us in the belief as to essential identity of nature. Otherwise my argument will break down, for to those who demand demonstrative proof I have but little to say. Taking, then, lupus vulgaris — a dermatitis of parasitic origin, widely infective in its early stages in some cases, and locally infective during very long periods in all—as the type of what we mean by the word Lupus, I have next to

ask that we do not restrict our conceptions of the maladies which ought to receive this generic name too exclusively to this form. Let us not be, when we speak of a lupus malady, for ever sending our minds back to the phenomena of lupus vulgaris. It is, in fact, only one of a large family which is associated, by a common origin, in close bonds of relationship. That common origin, if it be not actually the tubercle parasite, is, of course, in intimate connection with it. Our knowledge is not sufficiently advanced to permit us to be very definite in this matter. I would not call anything "lupus" without intending to imply that it was in some way associated with tuberculosis; but at the same time I would allow to chronic and persisting local infectivity very considerable weight as being itself probably due to such association. Now the clinical evidence which places lupus erythematosus in the same family with vulgaris is, for me, overwhelming, and in spite of the non-recognition of the bacillus by microscopists, I believe that it is even more closely associated with tuberculosis than is L. vulgaris. We have, besides, innumerable connecting links between the two, and notwithstanding the ease with which diagnosis is made between typical examples of the two, there are, as I think all will admit, some examples in which it is impossible to decide between them. The malady known by the old-fashioned name of "lupus sebaceus" stands as a sort of connecting link between them; it is usually an adjunct of lupus erythematosus, but is often solitary and locally persistent after the manner of vulgaris. The differences appear to depend not on difference as to primary cause, but as to the precise structure in the skin primarily attacked. In lupus erythematosus the infective period is both longer and usually of greater activity than in vulgaris; and thus multiplicity, and often symmetry, are more easily attained. New manifestations may continue to occur for several years, but this liability is not of indefinite duration. In all cases the process comes to an end after a time, a more or less complete spontaneous quiescence, or even cure, being the final result.

Are we not in some danger of being too precise in our diagnostic efforts. Do we not sometimes set ourselves im-

possible tasks, and attempt to distinguish between things which in reality are not distinguishable? Diagnosis should be based upon essential nature, and not upon mere external appearances; yet is it not true that we often allow ourselves to apply distinctive names to groups of phenomena which we have seen only from the outside. Like children at a theatre, we look too exclusively at the scene before us, and forget that its actors have been trained for their parts, and that the hero, the villain, and the victim may possibly be, when in undress, members of the same family. We should understand matters better were we less willing to be satisfied with what is shown us, and more zealous to look behind the scenes. Possibly I shall more nearly approach the language of science if I say that we are too eager to discriminate, and too slow to combine and to trace relationships.

Now lupus erythematosus is by no means the only form of skin disease which I should like to associate with lupus vulgaris as possessing infective properties which are in all probability due to some partnership with the bacillus of tubercle. I have just mentioned the lupus sebaceus of old authors, and I would, if I dared, put in a claim for infective lymphangeioma as being really a lupus lymphaticus, and also for a whole group of exceptional and mixed forms of disease which have as yet received no distinctive names. Provisionally, some of these may be known as acne-lupus, eczema-lupus, and even as nævus-lupus. Respecting the last, it may be remarked that congenital angeiomata do not, as a rule, manifest infective properties; now and then, however, they do so, and may continue to advance for many years, healing and leaving scars behind them exactly after the fashion of lupus vulgaris. Can any more plausible explanation be suggested for these extraordinary cases than that they are examples of nævus attacked by the tubercle bacillus. If this be not accepted, whence come their serpiginous and cicatrising tendencies. In one of the most remarkable examples of an infective and spreading nævus on record (Dr. Anthony Todd Thompson's case), the child died of tuberculosis of the membranes of the brain. The close simulations of lupus which we often see in the later stages

of syphilis—so close that even the most skilled observer may be in doubt—are probably cases in which the presence of the tubercle bacillus stamps with peculiarity a dermatitis originally syphilitic. They are partnership maladies, and the term "syphilitic lupus" is quite appropriate.

Some may, perhaps, think that I am going far ahead of the facts in suggesting that the presence in some form or other of the tubercle parasite is the source of the infective properties of most chronic and serpiginous inflammations of the skin. Time does not permit the full development of the argument by which such a creed may, I think, be sustained. It may be permitted, however, to hint that we are only on the threshold of our knowledge as to the habits and possible metamorphoses of this almost ubiquitous microbe. The facts which have been proved as to its possibilities of latency make it a probable theory that it is really present in very many in whom nothing reveals its existence. It can, we may reasonably believe, pass, in particulate form, from parent to child, and may, in such case, remain quiet for quite indefinite periods, until some depravation of general health or some local injury gives it a fitting opportunity to display its powers. It is a Proteus ever revealing itself in forms apparently different but really the same.

There are, of course, other forms of tuberculosis in which the skin is implicated, which ought not to be admitted into the Lupus family because in them the skin suffers only secondarily. Such are the ulcers with ragged undermined edges which occur in connection with tubercular lymphatic glands and tubercle of the subcutaneous cellular tissue. The affection which our late teacher M. Bazin described under the name Indurated Erythema of the Scrofulous, and which we now gratefully denominate Bazin's Malady, is an excellent example of this. There cannot be the slightest doubt that it is associated with tubercular tendencies, but inasmuch as the process begins beneath the skin and not in it, nothing in the least like any form of lupus ever occurs. Nor have I ever seen Bazin's legs in association with lupus of any other region. Thus we have another illustration of the law that the parasite keeps to the tissue which it has first

invaded, and that adjacent ones are implicated in common inflammation only.

Lessons from Dermatology as to the Nature of Cancer.

If I have made good the assertion that the skin offers an attractive field for the study of the tubercular process, my task is yet easier in reference to that of cancer. Using that term in its old sense as inclusive of all forms of malignant new growth, it may be remarked at once that the variety of those forms offered for our study by the skin is very great. Every year adds to the number of clinical varieties which it is well worth our while as specialists to distinguish from each other. That I may not weary you I will venture to state very concisely the general laws which seem to be illustrated by them:

1. That the malignant process has probably similar causes in all cases.
2. That the main predisposing causes are two—heredity and senility of tissues; and the exciting cause, local irritation or injury.
3. That the differences in appearance assumed by malignant growths are in direct relation with the special tissue in which the process has commenced.
4. That under the combined influence of local irritation and senility — indeed sometimes without the latter — the cancerous process may begin simultaneously in several parts at once, *e.g.*, multiple epitheliomata of face.
5. That chronic inflammatory or atrophic changes may often precede by a long time those which are cancerous, *e.g.*, Lip epithelioma from smoking; Cancer in Kaposi's malady; and Lupus-Cancer.
6. That changes which are not inflammatory, but mere modifications of nutrition, may also long precede and be introductory to those of cancer, *e.g.*, Lentigo-Melanosis.
7. That the occurrence in the child of precisely the same form of cancer as in the parent (*e.g.*, melanotic sarcoma

and rodent ulcers) is sometimes so definite as to suggest that germinal matter must have been transmitted.

8. That for the most part transmutation occurs in transmission, the form of malignant disease in the offspring not being precisely similar to that in the parent.

9. That certain quite innocent growths (*e.g.*, Steatomata of the scalp), having been hereditary in several generations, may gradually pass into malignant ones.

10. That certain non-malignant forms of hypertrophic growth (*e.g.*, senile warts and cicatricial keloid) are in close connection with inherited tendency to cancer.

11. That whilst some forms of malignant cancer may remain local for half a lifetime, others manifest the most vigorous infective powers and multiply with a rapidity little short of marvellous.

12. That certain chronic inflammations of the skin, not malignant in the first instance, tend to develope cancerous processes of peculiar type (*e.g.*, Granuloma fungoides).

13. That scrofulous tendencies are no bar to malignant action (*e.g.*, Lupus-cancer).

Such, Gentlemen, are some of the general propositions respecting the cancerous process as displayed in its manifestations on the surface of the body which, did time permit, I should like to propound and to illustrate. It will be evident that taken together they lend no support to the hypothesis that cancer depends upon the introduction of any parasitic microbe. Rather they point to its alliance with inherited or acquired, general or local, disturbances of nutrition, and, above all, to reduction of that wonderful controlling force under which individual parts and structures are restrained in their growth within the limits of what is necessary for the good of the whole. Parasites there may be, for they easily develope in damaged tissues, and possibly complicate the processes in almost all forms of inflammatory action, but that any given parasite is the cause of cancer seems, in the light of what is observed in malignant affections of the skin, incredible.

May I add that this theory, asserting as it does identity

of chronic inflammatory processes with those of malignant action, cuts at the root of all hope of diagnosing by the aid of the microscope the earliest stage of cancer. There is always a precancerous stage concerning the peculiarities of which histology can say nothing with certainty.

It would of course be easy to cite many more illustrations of the services of Dermatology to general medicine. I might allude to the work done by the late Dr. Hilton Fagge in numerous directions, and that also of one whom we have yet more recently lost, the able and accomplished Dr. Leloir. I might remind you of the elucidation which diseases of the liver have received from the study of xanthoma, and especially of that peculiar form which is in association with diabetes, and upon which the writings of Mr. Malcolm Morris are noteworthy. I might connect sclerodermia and Raynaud's malady with the name of Dr. Barlow, and the gangrenous affections of the skin occurring in varicella and other constitutional conditions with that of Dr. Radcliffe Crocker. The knowledge that very peculiar and fatal forms of dyscrasia are met with in association with a general tendency to a bullous eruption and overgrowth of papillæ we owe to the researches of Neumann of Vienna, whose absence to-day we all regret. Lastly, that I may not weary you by the enumeration, I must be content to say that general pathology must ever rest indebted to Dr. Unna for his crowning and monumental work on the Morbid Histology of the Skin.

It is perhaps fair to claim for the English school that amongst us skin diseases and general medicine have never been disassociated. We have had, it is true, our Willan, our Bateman, and our Wilson, but amongst their contemporaries there were always hospital physicians in general practice by whom dermatology was scarcely less cultivated. The names of Dr. Addison, Sir William Gull, Dr. Hilton Fagge, and now Dr. Pye Smith, Dr. Taylor, and Dr. Perry almost enable me to assert that Guy's Hospital physicians have always considered it their duty to study skin diseases. It has been more or less so also at other hospitals, and greatly, I think, to the benefit of the British practitior

and the British public. In making this remark I hope that I am doing no injustice to other countries of which I have less knowledge, and in which possibly the same state of things may exist.

Classification and Nomenclature.

There remain yet two topics, both of so much delicacy, so beset with thorns, that I feel sure I shall be unforgiven if I altogether avoid them. I refer to attempts at reform in Classification and Nomenclature. Amongst the greatest benefits which Dermatology could confer on Medicine, would be the production of a scheme of Natural Classification of Diseases. It would be easier to do this in our department than in any other, and although it would of necessity be to some extent special, yet in all main features it would be applicable to general nosology and pathogenesis. Now although it may with truth be urged by prudent men that our knowledge is not yet sufficiently advanced to enable us to substitute for the more or less arbitrary schemes now in use a perfectly natural classification, yet it has sometimes occurred to me that much might usefully be effected by an orderly arrangement of recognised causes. Without presuming to locate definitely all the different maladies with which we are familiar, we might make beforehand suitable places for their reception in the future. Permit me to try to illustrate what is meant.

By a division as definite as that by which zoologists separate Vertebrates from Invertebrates, we might put apart the morbid processes which begin and continue as local and external from those which are caused by internal disorder. In the one we should obviously place such maladies as Scabies, Tinea, Molluscum, flea-bites, Porrigo, and the like; but about many others there might still be some doubt. These latter must wait and take their place when knowledge is more advanced. Undoubtedly many local diseases induce disturbance of general health, but when that is the case no difficulty ought to be found in detaching the primary malady from its complications, and assigning each to its proper

place. This primary rule of classification would at once condemn the practice of including under the same name diseases which obviously originate from totally different causes, as is now done, for instance, in the use of the term Infantile Pemphigus for an eruption (Porrigo) which is wholly of an external nature.

All schemes of natural classification must be based upon recognition of causes, not upon similarity of outward appearances, or even of histological changes. We must extend our purview to the whole clinical course of the disease, and above all must seek to gain some conception of its essential nature. With these aims in view no one with fair knowledge would have much difficulty in constructing a long series of natural groups, which would include most of the material with which we have to deal. There would remain, however, even in the hands of the most skilful, a certain residuum of facts which he would find it for the present impossible to place. These must wait.

It is perhaps not known to whom we are indebted for the first suggestion that in Herpes Zoster we have a form of neuritis, and not a primary skin disease. Whoever made that observation placed scientific medicine under great obligation. From the hint which he gave us a new family of diseases has been constructed, the limitations of which are as yet not clearly discerned. Amongst the maladies which take Zoster as their type, although externally very different, we now rank with confidence those localised forms of Sclerodermia, with which the names of Thirial and Addison, as original observers, are connected. To our knowledge of these Sir Erasmus Wilson and many others have made valuable contributions; and we now recognise that definite deviations from bi-lateral distribution, with other features which they have in common with Zoster, prove that they are evoked by the cutaneous nerves. Other non-symmetrical affections, such as the congenital streaks—in connection with which I must mention Dr. Stephen Mackenzie's name—although they have not yet been proved to be of similar origin with Zoster, yet receive important elucidation from what we know of it. That Herpes Zo

may be caused by arsenic is now, I believe, an accepted fact, and from it we are justified in the general assumption that drugs introduced into the circulation may produce local neuritis. Between typical Herpes Zoster and Herpes labialis, whilst we recognise their differences, we find also many strong links of connection, and we avail ourselves of known facts in reference to both in our attempts to appreciate the real nature of the disease which Dr. Duhring has given us under the name of Dermatitis Herpetiformis. Respecting the nature of Herpes, those who are engaged in syphilitic practice have important evidence to offer. They tell us that a liability to recurring attacks results from any local inflammation;—an indurated sore or a non-infecting one, or even from a mere gonorrhœa. They tell us further that syphilis unquestionably predisposes to recurrent Herpes, and that herpetic affections of the mucous membranes are very common. To these facts it remains only to add that muscular paralyses occasionally result from herpetic neuritis, and it is, I think, proved that the nerve-located affections of the skin constitute a natural family of the utmost importance.

The student of natural groupings in reference to sameness of cause cannot fail to have his attention strongly drawn to the part played by congenital peculiarities of structure as predisposing influences. Here again we may claim that Dermatology helps Medicine. We have it demonstrated in the most conclusive manner—and, let me add, our observations are confirmed by those of the ophthalmologist in reference to such maladies as retinitis pigmentosa—that in certain families several individuals may be born with peculiarities of the skin not revealed at birth, but destined to become very evident on exposure to the ordinary surroundings of life. They are born, indeed, with skins not well made and not likely to wear well. Many illustrations in proof of this might be quoted, but by far the most conclusive and instructive is that which will, I hope, commemorate in its name the great services to dermatology which have been rendered by my distinguished predecessor in this presidential chair. I allude, of course, to Xerodermia pigmen-

tosum, or Kaposi's malady. The chapter in pathology which is written for us by this singular affection is full of interest. A congenital susceptibility of the skin to the influence of light, producing first lentigenes, then ulcerations, next scars, and lastly fungating cancer of scars,—such is the order of events in severe and well-marked cases. If there be several children in the family it is almost certain that more than one will suffer. Taking our cue from what is here so conspicuously displayed, we may assume that inborn peculiarity of the skin is at the root of many of its obscure diseases, and that these are often not revealed at birth, but await their appropriate exciting causes. The test is, do they occur in several members of the same family? for all that is inherited ought to be shared more or less, though possibly very unequally, by the offspring of the same pair. The judicious application of this general law will, I feel sure, afford us help in many a difficulty as to classification.

In close relation with those just referred to we must constitute another large and perfectly well-defined family, in which some wholly inappreciable inborn peculiarity renders its subject liable to functional disturbances of a special kind. Idiosyncrasy of function, if we may use the term, must be evoked to explain many and severe forms of disturbance which result from exciting causes, apparently of an insignificant nature. Dr. Prince Morrow, in his Manual on Drug Eruptions, has given us an excellent synopsis of most curious information on this subject. The fact that one patient may be covered with a frambœsial eruption as the result of a few grains of iodide of potassium, whilst another may swallow as many drachms with entire impunity, is only one of a large class. What is true of the iodide is true in greater or less degree of many other drugs, and also of some articles of diet, and of certain condiments. It is especially when maladies are recurrent, sudden in development, prone to subside rapidly, and apparently causeless, that we must be especially alive to influences of this kind. A few fragments of a herb usually so harmless as parsley may, in case of idiosyncrasy, be the cause of plentiful urticaria.

Whoever may make the attempt to classify on natural principles not so much diseases as the causes of disease, will be met at once by the difficulty that these causes are very frequently by no means in solitary action. He will have to recognise that there is nothing to prevent the possessor of the tubercle bacillus from receiving the virus of syphilis, and it will become at once a question requiring careful examination as to how far and in what ways these two pathogenetic influences may modify each other. So also may the subject of some inherited idiosyncrasy suffer from the injurious influence of some other of the numerous causes of disease. The possible combinations are multiform and innumerable. I must not venture to do more on the present occasion than just hint that it is under the laws of pathogenetic partnership that we must seek the elucidation of a large majority of the complex phenomena which are submitted to our investigation. But few diseases are really simple and uncomplicated in their causation, and it results that but few are simple in their nature or uniform in their developments. Such terms as "sui generis," "morbid entity," and the like will find but little employment in the language of the observer who is zealous in the study of causation. We have, it is true, a certain number of agencies which may take rank side by side with the simple substances of the chemist, but even these, when acting on the living tissues, soon find themselves in combination with, and to some extent under the control of, other agencies.

A few words as to *Nomenclature*. Names are good servants, but bad masters. Nothing is more helpful than a well-devised name which crystallises out into one word, or at most two, all the salt of a whole page of description. All honour to the men who have invented such. They have cleared our mental processes, helped our memories, and made easy the mutual interchange of opinion. If, however, we allow names to assume a false position, to represent imaginary actualities which they do not and cannot define, they become at once our masters and our tyrants, and may greatly impede our work. Realising these

dangers, there have doubtless occurred to most of us moments in which we have been willing to pronounce anathema on some friend who has promulgated a new name. Yet in an advancing science like our own new designations for the results of new observations are inevitable. May we not go further, and assert that they are much to be desired, and that although our art may have suffered somewhat from the injudicious multiplication of names, it has received far greater loss from unwillingness to incur blame in this matter. Let us use our names lightly, and they will do us no harm. A far more detailed classification of our facts seems to me most desirable, and in order to effect it we must devise many new names. The necessities of a too restricted nomenclature have in the past made strange bedfellows. There is nothing in what I now say in any degree inconsistent with what has already been urged against too strenuous insistance on artificial discrimination. There are risks of error on both sides. The tasks before us involve perpetual attempts at both analysis and synthesis, and we must not allow ourselves to become weary in the ever-recurring work of separating and putting together again, distinguishing and recombining.

Clinical Groups.

May I be permitted, without incurring the charge of egotism, to illustrate my meaning, and possibly to add to the interest of my discourse by briefly describing one or two recent attempts of my own at name-giving. In certain rare instances the common encysted tumour of the scalp (steatoma), having existed hereditarily in several generations, gives rise to multiple solid tumours, which may occur on other regions than the scalp and run a malignant course. I possess portraits from three such cases so much alike that they might be mistaken the one for the other. The first on record was described a quarter of a century ago by Dr. Ansell in the Transactions of the Royal Medical and Chirurgical Society, with a good portrait and an account of the autopsy. So far as I am aware no one else has published cases, and Dr. Ansell's observation was in danger

of being lost. The clinical fact which the cases illustrate is one of great interest, and although they are no doubt exceedingly rare, they differ from all others, and deserve separate recognition. The three portraits have been placed together in one frame, and have been named STEATOM-SARCOMA: THE ANSELL GROUP.

In another instance I have ventured on the name "SENILE LENTIGO-MELANOSIS: THE ROLLO GROUP." Now Rollo is the pseudonym of a lady who supplies the best example of the malady, and who has been good enough to sit for its portrait. Her case has been fully described in print, and can be read by any one. By the use of her name it is intended to imply that no other cases should be placed in that group but those which closely correspond with hers. Her portrait has been published, and any one who likes can possess it. The composite noun Lentigo-Melanosis is justified by the fact that the disease begins as freckles and progresses as melanotic staining, which may extend widely. In association with the staining in its late stages, sarcomatous or epitheliomatous growths may occur. The addition of the term "senile" is required because it is the lentigines of old persons and not those of youth which are prone to take on these processes.

To another group of cases in which the features (and presumably the nature) of chilblains, acne, psoriasis, and lupus are inextricably mixed, I have preferred to apply simply a patient's name, *Philip Holmes*. In yet another, in which inherited gout and Raynaud's liabilities are the chief causes, the term "Mabey's Group" seems more convenient than any name based upon pathology. Portraits both of Philip Holmes and of Emma Mabey have been published, with full details of their cases and those of others belonging to their respective types.

It will naturally be objected that if names which convey no meaning are to be thus assigned, they will be useful only to the observer who confers them, and that to all others they will bring simply bewilderment. The reply to this is that such names, and in fact the knowledge of the phenomena which they designate, are not designed for everybody's

use. They are only for the initiated and those wishful to go into detail. There is no royal road to such knowledge, nor any royal aids to its retention in the memory. Those who want it must perforce go to the place where it is recorded, and must be more than satisfied if they find it there given with clearness and precision. Our zoological friends have long been at their wits' end to find appropriate names for their rapidly increasing discoveries, and not very long ago a newly observed inhabitant of the ocean was gravely named "Golfiana" for no other reason than that its discoverer was on a golfing holiday when he found it. Although it is to be admitted that the subjects with which we deal do not divide themselves into genera and species, yet it is not more needful to the convenience of the entomologist to give distinct names to every species of beetle, than it is to us to separate into appropriate groups, with appropriate names, the multitudinous affections displayed by the skin. The employment, perhaps only temporary, of the patient's name (modified where concealment is desirable) appears to offer a very convenient method for designating these groups—one, let me add, which the entomologist may envy.

In the bestowal of names we must be content to submit cheerfully to the law of survival of the fittest. The well-known anecdote told, I think, of Beau Nash is here appropriate. His valet, who had been dressing him for a reception, is said to have been met coming downstairs with an armful of slightly crumpled cravats, and to have remarked, "These are our failures." So we must expect many "failures" in our efforts at improved nomenclature. Some of our names will encounter disapproval, others will be forgotten; our friends will rechristen in inadvertence children which we had thought our own. All this must be taken in good part, remembering that the disappointment is only temporary, and that the outcome will be the advancement of knowledge.

In suggesting the employment of the patient's name, it may be well to remark that it should not be used as that of a disease, but only of a special group of cases. The honour of giving a name to a disease should be reserved for members

of our profession, and should, for the most part, be restricted to those who are gone from us. It may be added that we want new names not so much for substantive diseases, of which there are but few, as for well-specialised groups, of which there are very many.

The Social Importance of Dermatology.

In what I have been saying, the more immediate aim has been to claim for Dermatology its rightful place as a field for the observation of morbid processes and the study of the nature of disease in general. Little or nothing has been said or hinted as to its immediate importance as a means to the mitigation of human suffering. Yet this aspect of our calling must not be wholly passed by in silence. It is too much the fashion in certain quarters to assume that diseases of the skin are trivial, or at any rate of less importance than those of internal parts, or those for which the surgeon performs his brilliant operations. It is needless to say to you that such a notion is the offspring only of ignorance and defective sympathy. In truth, no class of maladies cause more distress than those which we combat. It is not without significance that the final trial to which the patience of Job was subjected is represented to have been not a diabetes, nor a rupture, nor even a cancer, but a loathsome disease of the skin. Probably there is no more distressful mode of death than that from pemphigus vegetans; and if we take into account the social degradation which it involves, what malady has caused more misery than has leprosy? Of the affections which are not mortal, many yet, if uncured, may mar the prospects of a life. Lupus has spoiled the fortunes of many a fair face, and were it not for the timely aid of the dermatologist other forms of chronic skin disease would pronounce the doom of reluctant celibacy for thousands.

The mention of leprosy leads to the remark that we have yet problems of world-wide interest before us. I do not know that we can claim vaccination or the exanthems as belonging in any special sense to us, but Leprosy, Frambœsia, and Pellagra certainly await our verdict. Each one of the three is the cause of untold wretchedness. Of

Pellagra, I suppose we may say that the problem has been solved, and that, in associating it with the use of diseased maize, the means for its prevention has been made fully known. As to Leprosy and Yaws we are still in disagreement. I may confess that I personally regret that neither the one nor the other of these will come before our Congress as a subject for formal discussion. They are topics which ought, I cannot but think, to be triennially revived amongst us until they are set at rest. It is possible that I may be self-confident in my conclusions, but believing as I do that abstinence from imperfectly prepared fish would wholly prevent the one, and that the judicious employment of modern methods in the use of mercury would reduce the other to insignificance, it follows that I am most anxious to use every opportunity for promoting inquiry and removing doubt. Heartily do I applaud the zeal of those who devote their time to minute investigations into what are apparently only minor matters. We never know from what quarter light may come. By all means let us take such tithe as we can get of the mint and the anise and the cummin, but let us never forget to put in their proper place the weightier matters of the law. The two maladies named have surely in the present day a foremost claim upon the conscientious and painstaking attention of every dermatologist.

Now, gentlemen, in conclusion I may just remind you that it is not unusual to commence the sittings of a scientific congress by a religious service. We have made no arrangements for any such. Nor would it, I think, have been suitable to have done so. Those whom I see before me represent many and very various creeds. Many of us have probably accepted in all its fulness that article of faith which teaches us that to work is to pray. As, however, you have appointed no chaplain, will you pardon me if I, as your president, for the moment take upon me his function. I wish to express to you, and for you, the earnest desire that we may conduct the affairs of this Congress—repressing, as far as possible, all selfish motives—with a single and devout aim at the discovery of truth, for the advancement of science and the increase of the happiness of mankind.

VACCINATION NOTES.

No. I.—*An apparently healthy Vaccinifer the means of conveying Syphilis—Infection of the mother's nipple—Good illustration of Colles' Law—Account of the state of the Vaccinifer twenty-five years after the occurrence.*

Mr. Ward, of Leeds, in his evidence before the Commission stated that he had known of a case in which two children received syphilis from vaccination. The occurrence was not recent, but had taken place about twenty-five years ago. In the course of inquiry as to the facts it came out that the vaccinifer was still living in Leeds, and by the wish and at the expense of the Commission, she and her mother were brought up to London for inspection. Mr. Holmes, a public vaccinator at Leeds, the surgeon who had performed the vaccination and who was well acquainted with the facts, was good enough to take much trouble in assisting the inquiry, and himself accompanied the patient to town. The case presented facts of the utmost interest, and these I now purpose to record. Mr. Holmes and the patients attended at my house on July 2, 1896.

The vaccinifer, now aged 26, is a well-grown and healthy-looking young woman. There is nothing in her physiognomy which would in the absence of knowledge of her history excite suspicion; but, on inspection, there are definite radiating scars at the angles of the mouth, the skin of the cheeks is softer than usual, and the frontal eminences are developed in some slight excess. One of her upper central incisors has been destroyed by caries, but the other is of good size and shape. The lateral incisors are very small; the lower in-

cisors, canines, and bicuspids are all normal. Almost all the molars in both upper and lower jaws have been destroyed level with the gum by caries. She is not in any degree deaf, and she has never as yet suffered from keratitis. No traces of choroiditis can be found with the ophthalmoscope. In spite, however, of this absence of evidence, Mr. Holmes assured me that almost immediately after she had been used as a vaccinifer she showed a most unquestionable syphilitic eruption and had sores on the nates. He treated her with mercury, and under it she recovered. In complete corroboration of this statement, I found, on asking to see her legs, that the left leg was seamed with deep scars. These were attributed to the results of a kick five years ago, which did not break the skin. For the ulcers on the leg she had been long an inmate of the Leeds Infirmary. It was clear that the cellular tissue had been deeply affected, and that the sores had been ulcerated gummata. It was not certain that there had been any periostitis, but the tibia felt thick. The other leg had suffered nothing.

Mr. Holmes told me that at the time that he employed this patient as a vaccinifer she was under three months old, and had, so far as he observed, the appearance of being a healthy child. She was the first-born of her mother's second marriage. The mother was at the time in good health, and had remained so (with an exception to be noted presently) ever since. About a year after the child's birth her father was drowned, leaving her mother one month pregnant. Nothing was known as to the father's illnesses. His wife considered him a healthy man. His second and posthumous child died within a year of birth, probably from inherited taint ("with ulcerated mouth, &c."). In both these two children vaccination had proceeded quite normally.

Thus, then, we appear to have proof—1. That an infant who appeared to be healthy, but in whom syphilis was latent, might afford a tainted vaccine lymph capable of producing the disease. 2. That the vaccination might proceed quite favourably in the vaccinifer. 3. That under suitable treatment the tainted infant might completely recover and grow into an apparently healthy woman. 4. That the

usual indications in adults of infantile taint may be almost entirely absent. 5. That large tertiary gummata may, in the subject of an inherited taint, form in adolescent age without other symptoms. 6. That in a woman who was known to have taken a course of mercury in infancy all the molars may be destroyed by early caries, whilst the bicuspids and, for the most part, the incisors may escape.

As to the children vaccinated. Six were vaccinated from this vaccinifer, and of these, two had chancres form in the site of the vaccination pocks, whilst the four others wholly escaped. Respecting one of the two, no information beyond the fact that a chancre and eruption resulted was to be obtained. Mr. Holmes wholly lost sight of this child soon after he had made this diagnosis. The other child suffered severely, and, as Mr. Holmes believes, its mother much neglected the treatment. She became the subject of tertiary affections, and finally, at the age of sixteen, died with abscesses on the skull bones and ulcers in various parts. Whilst nursing this infant its mother contracted a chancre on her nipple.

These statements conclude the history of the vaccination, but there remain yet some facts to be mentioned as to *the mother of the vaccinifer* which are not without their interest. This woman had been married three times, and had children by all her husbands. She is still a comely and healthy-looking woman. She knew all about the subject of our inquiry, but assured me most positively that she had never suffered in any way. During her second marriage, as well as before and since, she had enjoyed excellent health. She nursed the vaccinifer at the time that the latter communicated the disease to the other infants, but she never contracted a sore nipple or had other inconvenience.* I observed that she had three conspicuous scars on her nose, and on inquiry as to these she said that she had been operated upon for "lupus-cancer" at an infirmary. This was only a year ago. The scars, however, were not like those of lupus vulgaris. There were three in number; not continuous, though ad-

* An example of Colles' Law which would meet all requirements, for the child was proved to be contagious in vaccination.

jacent, and they were supple and perfectly sound. I have no doubt they were consequent on syphilitic lupus, and this impression was confirmed by her statement that the so-called lupus had been present only a few months and that the nose had "inflamed like erysipelas."

From her first marriage this woman had a child, still living and quite healthy. The two born of her second marriage were, one the vaccinifer, and the other the child who died at ten months presumably from inherited syphilis. After a two years' second widowhood she married a third time, and bore subsequently seven children, of whom five are living, and concerning not one of whom has any suspicion ever been entertained. Yet, as we have seen, after more than twenty years since her pregnancy with the two syphilitic infants, and after having enjoyed excellent health all the time, this woman became ultimately the subject of syphilitic gummatous ulcerations on her nose. It is a curious and instructive fact that mother and daughter both suffered for the first time from tertiary symptoms more than twenty years after the birth of the latter, both having been in the interval quite healthy.

No. II.—*An attack of Erysipelas in a pregnant woman—Erysipelas in her infant after vaccination, the vaccine pocks being unaffected.*

The following is a narrative supplied to me by Dr. Sloman, of Farnham, by whom I had been consulted during the progress of the case. I never myself saw the patient. All that relates to the occurrence of erysipelas after vaccination is of great importance, and in this instance the development of the inflammation at a distance from the pocks renders it especially so. There are many well-known facts which make it probable that the virus of erysipelas may remain long latent, its evolution waiting for some exciting cause. This is the hypothesis by which we explain the recurrences of attacks in those who have once suffered. In the present instance the mother of the patient had suffered from erysipelas during her pregnancy, and if it be not thought

too bold an imagination, the suggestion which I should like to make is, that the microbes then passed into the tissue of the fœtus, which produced the attack after vaccination. It is possible that it was a mere coincidence, and that the erysipelas of the vulva would have occurred just the same if no vaccination had been done. On the other hand it is not improbable that a slight febrile disturbance, resulting from the vaccination, favoured the development of the erysipelas.

It will be seen that the attack of erysipelas was both extensive and severe, though restricted to the skin.

The following are Mr. Sloman's notes :—

"I shall most gladly give you any particulars I can about this very interesting case. The child was born on September 17, 1895, a perfectly healthy child of medium size; the mother, aged about 21 years, also very healthy, and after a very easy and simple labour. The mother had an attack of some kind which was of an erysipelatous nature when in India when she was about six weeks to two months pregnant, affecting the face and head principally; was laid up in bed eight days, and confined to her room for about fourteen days in all. On the 17th of October I vaccinated the child in four spots on the left arm, with a lancet, from human lymph, the vaccinifer and its parents being perfectly healthy and free from any known disease, or tendency to disease. The vesicles began to take in the ordinary way about the third or fourth day. On the twentieth the child seemed poorly, was very sleepy, and had been sick once, with temperature 101° F. The following day (21st) the nurse noticed a hard red swelling of the right labium pud.; the next day it spread to the buttock, and thence, at intervals of twenty-four to twenty-eight hours, over the whole of the limbs and trunk, including the face and back of neck. All this time the vaccination vesicles followed the ordinary course of development, rupture and scabbing, the erysipelas passing down the vaccinated arm about the fourteenth to eighteenth days, but in nowise affecting the vesicles. The scabs came off on the eighteenth to twenty-first days, leaving absolutely healthy healed scars underneath. The erysipelas affected the whole of the true skin, and in some places the subcutaneous tissue, but in no instance threatening suppuration. Before each spread there was great sleepiness, with rise of temperature to 104° or 105° F. and great depression of pulse. I tried various applications, but found Ichthyol certainly the most useful, using the Ichthyol Collodion 10%. Internally I used tincture of steel, three-minim doses every two hours, and ʒj of brandy in milk or very weak Valentin's juice every one to two hours, according to state of pulse. At the end of six weeks the child was well. No contagion occurred to others."

A volume on vaccination which is in preparation for the New Sydenham Society will deal at considerable length with the subject of erysipelas. Some very interesting facts in illustration of the pathology and clinical history of this type of the inflammatory process are met with in vaccination practice.

A very interesting summary of facts in reference to the transmission of micro-organisms is to be found in the fifth volume of "Science Progress," from the pen of Dr. Buckmaster. Amongst other experiments making probable the transmission of infective elements to the ovum, those of Gärtner are mentioned. He inoculated canaries with mammalian tubercle, and some weeks afterwards took their eggs, and having washed them in solution of corrosive sublimate, introduced their contents into the peritoneal cavity of guinea-pigs. The result was that in two instances tuberculosis was set up.

There is nothing in the least improbable in the supposition that the microbe of erysipelas may pass by placental infection to the fœtus.

As regards the possibilities of long latency many facts might be quoted. Perhaps some of the most convincing are those in which a patient undergoing an operation many years after an attack of erysipelas suffers another attack of it in spite of the most perfect antiseptic precautions. Of this in the case of excisions of the breast after erysipelas of the head and face I have myself seen several examples.

MYOSITIS OSSIFICANS.

I REVERT to this subject in order to correct an erroneous diagnosis which was unfortunately published in my former paper. In that report the particulars of a case were given in which it was believed that the pronator radii teres was in process of ossifying contraction in both forearms. The result has proved that, although contracted, the muscle is not ossified. This was revealed first by the new photography, and subsequently by cutting down on the muscle. On the latter occasion its tendon near its insertion was cut through, and it was made quite certain that there was no bony matter in it. In excuse for my mistake I may say that the muscle in question was so hard that it deceived all who examined it, and many besides myself had done so. No other muscle in the forearm was contracted. It still remains very difficult to explain the condition of the boy's forearms. The right is fixed in complete pronation, and the left appears to be gradually approaching that condition. Skiagraphy, whilst it has established the negative as regards the absence of ossification, has not given us any further assistance. It shows the bones in extreme pronation, and that is all. The division of the muscle under full anæsthesia did nothing towards liberating the radius.

I will take this opportunity for appending an abstract of another example of the malady which was omitted on the former occasion for want of space.

A Case of Myositis Ossificans.

The following is the description given by Dr. Weldon

Carter of a patient under his care (see *Lancet*, February 10, 1894) :—

A boy nine years of age first came under my observation in April, 1892, being brought to the Wigan Infirmary for certain "lumps in his back," which had been first noticed four or five years previously and had gradually increased in size and number during that period. There had never been any complaint of pain or tenderness. The patient's family history was good, and he had always enjoyed good health. The condition of the boy when seen by me on April 21, 1892, was as follows:—He was quite healthy in general appearance, though he stood somewhat stiffly with his head slightly bent forward, and was unable to fully extend the dorsal and lumbar portions of his spine; the cervical portion, however, presented very little alteration in mobility from the normal. On stripping the patient, the lumbar portion of the erector spinæ on either side was found to be converted into a mass of bone which projected backwards and laterally more than in the case of healthy muscles; the mass measured four and a half inches transversely—*i.e.*, two inches to the right and two and a half inches to the left of the middle line of the body. The sacral spines were rather more prominent than natural. In the posterior axillary folds there were observed irregular movable masses of considerable size, which, though more prominent on the left side, occupied a greater length of the axillary border of the right latissimus dorsi muscle. Other movable masses were found in the teres major muscles, along the vertebral border of the right scapula, near the angle of the left scapula, and in the left trapezius muscle near its occipital attachment. The great toes were in the position of hallux valgus, the phalanges of the first, fourth, and fifth toes in both feet were very short, and there was slight webbing between the second and third toes of each foot. The hands were normal. On examining the boy on April 7, 1893, I found that there was no increase in the stiffness of the back, but the bony masses previously noted had undergone considerable increase in size, this being especially the case with those in the latissimi dorsi. There were several fresh bosses of bony deposit to the left of the spine, commencing at about the level of the eighth dorsal vertebra and extending outwards and slightly upwards in the direction of the left scapula. Several other new nodules were discovered in different parts of the back. So far as could be made out, there was no evidence of the adductor muscles of the thighs being affected. The masses of bone were mostly of an irregularly rounded shape, but a few were somewhat spiculated. There was no indication of any old or present suppuration.

Remarks.—This case supports the conclusions arrived at by Mr. Stonham that myositis ossificans occurs more frequently in the male sex, in youth, and without hereditary transmission; also that it commences in the muscles of the back and runs a progressive course. It is peculiar, however, in the entire absence of any history of local pains, nor is ther-

any history of such etiological factors as cold, damp, injury, or disease, which have occasionally been met with in this affection. The case is especially interesting as showing its association with hallux valgus and microdactyle, and I would add that this observation was made quite independently, for at the time of writing my original notes I was quite unaware of their occasional association, which greatly enhances the value of the observation.

NOTES ON LEPROSY.

Leprosy in Colombia—Management of Lazarettos.

A SERMON preached in July of last year before the Society of St. Lazarus, in Bogota, has been sent me by Dr. Ashmead, of New York. It contains some interesting particulars as to the reputed increase of leprosy in that district, and also as to the inefficiency of Lazarettos as a means of isolation. The Reverend Father thinks that lepers in the department of Santander have increased from 1,500 in 1888 to 5,000 or 6,000 in 1895, and thinks it possible that in the whole republic there are not fewer than 27,000. He says that whilst almost all the doctors say that it is contagious, all the lepers deny it. The management of the two large Lazarettos is evidently in the hands of non-contagionists. It may probably be taken as a fair illustration of what went on in the leper-houses of England in the middle ages.

"In both these leper-houses the healthy are much more numerous than the diseased; two healthy ones to one sick; this is the proportion of the population of the lazarettos. But what are the healthy people doing there? Some, the smaller number, probably attend to the patients; it is the healthy daughter who accompanies and attends her mother, or *vice versa;* it is a brother, some relative who does not want to forsake a sick brother or relative; to that there is nothing to say, it is a laudable act of charity which these healthy persons are performing, and God will not leave this work of love without reward. But, on the other hand, there are other healthy persons, the greater number, probably, who are there for other reasons. Some for the purpose of doing business, others with the infamous object of exploiting the poor lepers. On this subject, I have been told in the Lazaretto of Contratacion of incidents that were really scandalous. Moreover, in both lazarettos he enters who will, and remains as long as he likes; he may even establish his residence there if the whim takes him to do so, without any one having the right to interfere. . . . The attendants are generally healthy persons, who have to live in intimate contact with the diseased, eat at the same table, perhaps from the same plate, sleep under the same use the same garments." . . .

There are two ways of looking at these facts. It may be said that such carelessness fully explains the increase of leprosy, or, on the other hand, it may be argued that if there were any real indications that the disease could spread by contagion no one would be so foolish as to encounter wilfully such risks.

We may note that Spanish Colombia is a Catholic country, and that the fish-eating fast days are no doubt observed just as they were in England when leprosy prevailed here.

Recovery from Leprosy.

The Pictorial Atlas of Skin Diseases now in course of publication (the Rebman Company) by the staff of the St. Louis Hospital, is likely to prove a most valuable work. The plates, which are selected from the stores of the St. Louis Museum, are admirably executed. The annotations by its editor, Dr. Pringle, add much to its interest and usefulness. The letterpress descriptions, which are detailed, are all by one or other of well-known physicians who constitute the St. Louis staff. The last number deals with Leprosy. The subject of the portrait was a man who had been born in Guadalupe, but in whose family there was no previous history of leprosy, nor had he, so far as he knew, ever come in contact with any leper. He made a good recovery during a nine years' residence in France, and a portrait showing his appearance, after all cutaneous swellings had been removed, is given. It is in all respects a common type of case, a mixed form of disease, both anæsthesia and tubercles; no heredity; no contagion; residence in a leprosy district; gradual recovery during residence in Europe. The portraits are excellent. I do not think it worth while to specify the different measures of treatment adopted in this case, since precisely similar recoveries have occurred under the most varied management. I have had several myself. I always give arsenic, and at the same time a liberal diet, and if possible plenty of good wine. In one of my best cases the patient herself believed that port wine had cured her. The great point is, I believe, that the patient should change his diet, and cease to feed the parasite with half-cured fish.

A CASE OF FAVUS WHICH ASSUMED EXTRAORDINARY CONDITIONS IN CONNECTION WITH FEEBLENESS OF CIRCULATION.

In July, 1890, I was shown in the South Devon Infirmary, at Plymouth, a very remarkable case which, as I then thought, appeared to be, in some sort, a complication of Kaposi's disease with Raynaud's phenomena. At any rate, it seemed clear that the skin of the face and hands had a great tendency to inflame and ulcerate, with the formation of stigmata, &c., and that at the same time venous circulation in the digits was much retarded. Thus the whole of the face and head was covered with ulcerations and crusts. The hands also were similarly affected; whilst the feet, which were not inflamed, were in parts cold and livid.

The patient was a boy aged about 10, who was under the care of Dr. Clay. He had been an inmate of the hospital for upwards of two years, and his case had excited great interest. He came from a distant village in Cornwall, and no accurate facts as regards his family history were forthcoming, nor was it known exactly how the disease had commenced, nor at what age. His condition had been much benefited by careful treatment before I saw him. My visit was in the early part of July, 1890. At this time the boy's face was swollen and, on most parts, covered with pus crusts. These were especially thick over his nose, giving it the appearance of lupus. Almost the whole of his scalp-hair had been destroyed, and the scalp was in a condition, in parts, of scar and, in parts, of superficial ulceration. Much treatment had, I was told, been expended upon it by epilation, scraping, &c. From the description given

me, it would appear to have been at one time in a condition of diffuse kerion. The hair of the eyebrows, as well as that of the scalp, had been destroyed; but, curiously, his eyelashes remained unaffected. His lips were much swollen and inflamed, both on their outer and inner surfaces; and his tongue, which was covered with a white fur, was also red at its edges and showed deep sulci and some little excoriations. The skin was not affected lower than the upper part of the neck, and the only other parts involved were his hands. The skin of the lower parts of his forearms was mottled with stigmata. On the backs of his hands, especially on his knuckles, there were thick adherent crusts. All his finger-nails, with the exception of those of his ring fingers, were broken up and fibrous, the nail-bed being inflamed and the finger end somewhat swollen. In each hand the ring finger had wholly escaped, and its nail was still perfectly smooth. There was no evidence of increased pigmentation of the hands; but in reference to this, as on the face, allowance must be made for the fact that the skin was much concealed by crusts. Still I do not think the formation of pigment-freckles was a prominent part of the malady. On taking off the boy's stockings we found, although it was noon on a July day, that his toes and the outsides of his feet were blue from venous congestion. The feet were cold, but there was no ulceration of the toes or destruction of the nails. I could not get any very definite information as to whether the boy had ever suffered from ordinary chilblains, nor as to the time of year at which he had been worst.

It seemed clear that we had to do with a case of defective nutrition in the exposed parts, depending upon a very feeble circulation. Whether the inherited peculiarity of structure was definite enough to justify us in classing it as an example of Kaposi's disease would depend upon the family history, which, as has been seen, was not forthcoming.

The above notes were written out from memory immediately after my return from Plymouth, July, 1890. Dr. Gifford Nash, the house surgeon of the Infirmary, who had

taken great interest in the case, was kind enough to send me, subsequently, full notes of it. I state the chief facts in the following abstract:—

The boy, whose name was Abel Miners, was aged 11, and lived at St. Newlyn, East Cornwall. He was first brought to the Devonport Hospital on November 27, 1888, and the statement then given was that he had suffered from the eruption eighteen months. It was said to have begun "as a pimple in the front of the right ear," which extended and formed a sore. Near it others formed and became covered with "heaped crusts, like an oyster shell." The boy was one of ten children, of whom five were living and quite healthy. On this occasion, the boy remained in the hospital from November to the following March, when he left greatly benefited by careful local treatment. He had used various ointments. It had been noted that his tongue was red, excoriated, and deeply fissured. Great difficulty had been encountered in getting rid of the crusts, and it had been observed that when they separated a smooth surface was left which, in places, was raised above the level of the skin, but was not actually ulcerated. On April 9, 1888 (a month after his discharge), the boy was again admitted, much in the same state as on the first occasion, covered with crusts in the affected parts. He again used resorcin, the white precipitate, salicylic acid, creosote, &c. Both on this and the former occasion, iodide of iron and iodide of potassium were given internally. The fissures in the tongue were treated by applications of chromic acid in solution. In August, chloroform was given, and some of the affected patches on the left hand were scraped, and in November this measure was again resorted to for the face, scalp, and hands. Chrysarobin in various forms was used as an external application. On January 4, 1890, it was noted that the lower part of the left cheek was almost well, and that suppuration had occurred under two of the nails on his right hand. Fowler's solution in three-minim doses was ordered at this date. In February the boy had an attack of influenza, and the arsenic was omitted; but it was resumed after a month or two, and pushed to doses of eight minims.

Chaulmoogra oil was used as an application, and the parts were also soaked in solution of bichloride of mercury, after detaching the crusts with warm olive oil. Through the summer of 1890 the boy improved. As already stated, it was in July of that year that I saw him. Dr. Nash's notes in November record that he had fallen back. There was more secretion, the Chaulmoogra oil having apparently irritated. The scalp was still in a very boggy state. Some large crusts had formed between the scapulæ which, when detached, left a smooth red base. Another similar one had formed over the left loin. Up to this date the trunk had been free. The hands were still very cold, and at this date, all the nails, excepting those of the two ring fingers, were affected. The crusts or excrescences on the hands were very hard and horny. The feet and toes were blue and cold, but not affected otherwise. Up to this date no diagnosis had been made. The scabs had on several occasions been examined after soaking in liquor potassæ, and no fungus had been detected. When I saw him in July, there was nothing which suggested the diagnosis of favus, and I do not think that it even crossed my mind.

In January, 1891, arrangements were made to send the boy up to the London Hospital. Just before he was sent, Dr. Nash made another examination of the crusts after soaking in liquor potassæ, and for the first time discovered "long rods or mycelial threads exactly like those of favus." In writing to me of this fact he observed, "If, after all, it is a very bad case of favus, it will account for two cases of favus which arose in the ward during the lad's stay, and which I could not at the time explain the origin of." At this period he was twice injected with Koch's lymph, without any reaction being produced.

Towards the end of January the boy came up to London, and was admitted into hospital under my son's care. I did not myself see him for some weeks. I was told by my son that when he first arrived the peculiar odour of favus was distinctly present, and that the fungus was easily found in the crusts.

In March, I took the lad to a meeting of the Dermato-

logical Society. Every one who saw him remarked that there was nothing in his condition on any part to suggest the existence of favus, and at this time there certainly was no perceptible odour. The scalp, face, ears, &c., were still covered with crusts, swollen and sore. The tongue was still inflamed, red and fissured, and there were some white spots on the hard palate with surrounding redness, apparently due to touching the tongue.

At this date sketches of the face and hands were executed by Mr. Burgess for me. I had myself examined a crust removed from the skin of the back, and although it had not at all the appearance of a favus, it undoubtedly contained mycelial threads.

The boy's nails were at this time in a very peculiar condition. They were greatly thickened and very rugged. I expected to have been able to detach thick crusts, but found, on cutting their surfaces, that they were vascular and bled very easily. The nail substance itself was swollen and broken up by infiltrating fungus.

Although the fact had now been established that favus was present, we still had to ask whether the whole of the disease was due to that cause. Not only were the naked eye appearances quite dissimilar from favus, but the progress under treatment had differed widely from it. It had been found nearly impossible to get the sores to heal, and hard, almost horny, scabs had been very prone to form over them. There had been also, especially on the hands, a remarkable tendency to papillary outgrowth. The hands were covered with warty excrescences. Were we to consider that these deviations from what is usual in favus were due to inherent peculiarity of the tissues of the patient coupled with a remarkable feebleness of circulation and liability to venous stagnation? That the fungus could not be held to explain all, was further proved by the state of the boy's lips and mouth.

Conclusion of Case and Comments.

A month after the last note the boy, whilst still in the London Hospital, was attacked by pneumonia and se

He had for long been very feeble. Unfortunately, I was not informed of his death, and no autopsy took place. There was not, however, much left unsolved in the case, for there was no doubt whatever that the favus fungus was present, and had been the primary cause of the affection of the skin. A pre-existing peculiarity of feeble circulation (Raynaud's malady), with defective tissue nutrition, had in all probability been the cause of the very unusual features which the cryptogamic disease had assumed.

A good portrait of this boy may be inspected in the Cryptogamic Group in the Clinical Museum, and with it also sketches of his hands and tongue. In juxtaposition are many others showing the more ordinary conditions of Favus. Although the case is in my own experience unique, yet I have found in Cazenave's Atlas of Dermatology one that pairs fairly well with it. This portrait shows the head of a boy of about the same age as my patient. His face is scarred all over as if by lupus vulgaris, for which the malady would certainly at first sight be taken. His scalp is bald and also scarred, but not so deeply as his face. It is clear that the favus has been attended by ulcerative destruction and cicatrisation in parts. The crusts were no doubt carefully removed before the boy sat to the artist, and hence a feature in which the portrait differs from my own. Unfortunately the hands have not been depicted, and yet more unfortunately, I have been unable to find any printed details of the case. In all probability the boy was, like Dr. Clay's patient, of feeble circulation and defective tissue vitality.

DUPUYTREN'S CONTRACTION OF PALMAR FASCIA.

ALTHOUGH we are accustomed to speak of "Contraction of the palmar fascia," yet it is almost as great an error to suppose that the slips of the palmar aponeurosis are alone involved, as it was formerly to attribute the conditions to the tendons. Without doubt the cellular tissue thickens and forms adventitious bands, and sometimes even thick lumpy indurations. In some instances the skin itself is thickened and contracted. Nor, although it is usual for the condition to be met with in the ring and little finger, is it confined to them. The middle finger may be involved also, or even independently. In some instances a band is met with passing from the thumb to the index finger, which has become hard. Now and then bands of the same nature are found in the foot.

We must recognise, then, as the essential feature of this malady, a tendency on the part of the cellular tissue, aponeurosis and skin of certain parts of the hand and foot to thicken and contract. The questions needing decision are, what are the predisposing, and what the exciting causes? Amongst the former we may, I think, clearly place—

Hereditary gout.

Inheritance from those who have suffered from the malady itself.

The existence in the patient of gouty tendencies.

Amongst exciting causes we have:

Interrupted pressure on the part from the use of tools or playthings.

Injuries exciting positive inflammation.

Attacks of arthritic inflammation.

In many cases no special exciting cause has been present, the inherited proclivity having sufficed.

Whilst a very large majority of the cases have for their subjects adult men of more than middle age, yet the affection is not unknown in women, and is now and then seen in men comparatively young. Lastly, there is a congenital form, about which differences of opinion exist as to whether it has any connection at all with the other. The facts which I have to narrate will bear upon all these questions.

It is an interesting subject for inquiry whether, in connection with inherited gout or other causes, fibrous tissues in other regions of the body are liable to indurate and lose their elasticity. I have in former numbers of ARCHIVES produced cases which make it very probable that the well-known indurations of fibrous structures in the body of the penis, which cause curving of the organ and difficulty in copulation, are of the same nature, and that they are not very infrequently met with in the subjects of the palmar trouble. I am quite convinced that such is the fact. The original suggestion of this association was not my own, but was made to me by Dr. Carmichael, of Barrow-in-Furness. Many years ago I made the rash suggestion that one of the causes of glaucoma may possibly be contraction of the fibrous structure of the sclerotic, analogous to that which we meet with in these affections. Nor, although much has since been done by Dr. Priestley Smith and others to explain in other ways the phenomena of glaucoma, am I yet convinced that my hypothesis was wholly without foundation. Acute glaucoma and Dupuytren's contraction occur to much the same class of persons, at the same period of life, and in the same association with the inheritance of gout. It is by no means certain that the conditions which conduce to it are of a simple and uncomplicated character.

On the Form which is met with in Young Children and is sometimes Congenital.

There is a rare affection of the hands occurring in young persons, in which lumpy thickening of the skin, with some

lividity, persists for long periods. Of this the first recorded example was that of Dr. Judson Bury, but since his observations I have been able to publish several, and had adduced some facts which seem to connect the affection with inherited gout (see ARCHIVES, Vol. V., p. 237). Of this interesting malady Dr. Radcliffe Crocker showed an important example at the Dermatological Society two years ago. I expressed my belief that there would be found to be a history of gout in the family, and such on inquiry proved to be the case. When, on a second occasion, Dr. Crocker was good enough to show his patient in order to illustrate the disappearance of the lumps in the skin, it was observed that the patient, a young girl, was the subject of the congenital form of Dupuytren's contraction. Her little fingers were bowed forward by a band which had developed in their palmar aspects. Whether the condition was really congenital, or only of recent development, there was no trustworthy evidence. I was naturally willing to accept this observation as confirmatory of my creed in two directions: first, that these bands occur to the children of gouty parents; and second, that they give support of the conjecture that the conditions which they accompany are probably due also to gouty inheritance. Not long after I had seen Dr. Crocker's patient, I found in the records of the Northumberland and Newcastle Medical Society an important, though only partial, confirmation of these views. Dr. Morison there published the particulars of a case in which a little girl had this form of contraction symmetrically developed. She was the only child in the family who suffered, and there was nothing known as to the previous occurrence of Dupuytren's contraction in any relative. There was, however, reason to suspect the inheritance of gout, for her father had his hands crippled, and a paternal uncle had died of stone. Her mother also had the aspect of a free liver and was the subject of eczema.

Although in these and other juvenile cases the history of inheritance from ancestors who had themselves suffered from this special malady was not obtained, yet it must not b concluded that it was really absent. At the same time, v

may freely admit the probability that these contractions may develope *de novo* in those who inherit only gouty proclivities and not a special tendency to Dupuytren's contraction. It would appear that these bands may form in connection with rheumatic affections of the hands both in children and adults. I shall have to mention several cases illustrating this statement.

In discussing the nature of the association of Dupuytren's contraction with gout, we may note that it can only be supposed to be indirect. It depends not upon the deposit of urate of soda in the tissues, nor probably upon its presence in the blood, but rather upon a peculiarity in the proclivities of fibrous structures derived by inheritance from ancestors who were gouty. We find it in gouty families rather than in gouty individuals, and many of those who in middle life develope it as a matter of inheritance are probably themselves quite free from evidences of active gout. If this be true of what we observe in the form which occurs in middle age, it is much more emphatically so in respect to the congenital form. In the latter the connection with gout is probably yet more remote. The man who at the age of 45 developes, in connection very possibly with his trade, or his amusements, or his mode of living, this peculiar condition, betrays in so doing the fact that he had inherited from many generations of gouty ancestors fibrous structures which would not wear so well as they ought, but which were prone to harden and contract under continued irritation. If, however, this condition is developed in the unborn fœtus without waiting for the acquisition of gout, changes of age, or the influence of any local irritation, then it may be plausibly presumed we have evidence of something yet more definitely hereditary. We have probably—*pace* Weissmann—proof that some progenitor actually had, at the time that he became a father, some existing changes of this kind. It is from the damaged and contracted fibrous bands of his ancestor that the child inherits it. He could probably get it in no other way.

It will be a matter of great interest in the future to observe whether the congenital occurrence of palmar con-

tractions is ever observed as a family condition; that is, occurring to several members of the same group of brothers and sisters. It is a well known but very curious and important fact respecting these family disorders, of which ichthyosis, xeroderma pigmentosum, and retinitis pigmentosa are the best known examples, that there is seldom any proof of their being inherited. They are the results of some peculiarity in the parental conjunction which developes, in probably one half of the family, a tissue defect which had either never been observed in previous generations, or only in very modified form. Thus a congenital defect which occurs to only one in a family may be assumed to be more likely to be directly inherited than one which occurs to many. For the present, this is the position taken by the congenital forms of Dupuytren's contraction.

As regards these congenital malformations simulating Dupuytren's contraction, Mr. W. Adams holds that they have no connection with gout, or with inheritance from those who have suffered from true Dupuytren. On the other hand, he had constantly found them in association with hammer-toe, and believed that they were in some way dependent upon nervous disorder. In the first instance he believes it to be an inability to extend, and alleges that there is no real band in front or structural impediment to extension. At later stages, however, the condition much more closely approximates to the true Dupuytren state, and there are distinct bands which may require division.

Although Mr. Adams has not recorded any history of hereditary gout in his cases, yet he tells me that his patients have usually been from families in which it is very probable that such a history might exist. As to the coincidence of the affection with hammer toe, it may be that the latter is itself a quite parallel condition, and due to a similar cause, viz., contraction of fascia. Mr. Adams is in the habit of rectifying the position of the hammer toe by precisely the same means that he resorts to for Dupuytren's contraction, and in that way has succeeded in putting toes quite straight, for which amputation had been advised.

I will next briefly record a few facts illustrative of wha

I have asserted as to the ordinary form as met with in adults.

Dupuytren's Contraction in three generations and many Relatives.

An instance of the inheritance of tendency to Dupuytren's contraction was brought under my notice in the person of Dr. G——. In him the condition commenced at the early age of 36. He told me that his father, grandfather, and several other relatives had had it. He was not aware that any one had shown tendency to it congenitally.

Dupuytren's Contraction in several Relatives.

A man, aged 56, who had several times had gout. Conditions quite symmetrical; ring finger drawn into palm, little finger not contracted. A band developed in each hand from forefinger to thumb.

An uncle and a brother stated to have the same.

Symmetrical Dupuytren's Contraction of the palmar fascia in a gentleman of 64.

Mr. C——. This patient attributed the beginning of his affection to his having, on account of a sprained ankle, been obliged to use a stick. This was several years before I saw him. It began in the right palm, but soon afterwards appeared in the left also. He was the subject of a severe chronic eczema.

Dupuytren's Contraction of palmar fascia—Curving of penis.

Dr. F——, aged 68, has symmetrical contraction of palmar fascia, and has also a lateral curving of penis in erection. His father and grandfather had contraction of palmar fascia, and the latter had gout.

Dupuytren's Contraction in association with Gout.

Mr. B——, of Chester, aged 57, who had inherited gout and had himself had two attacks, came to me in July, 1893,

on account of an attack of urticarious erythema. It had occurred one night recently, and involved the whole body; so that the whole skin became scarlet. He was the subject of Dupuytren's contraction of the palmar fascia in an early stage. Only the right little finger was much affected, and it presented a bossy lump of thickening scarcely at all contracted.

Dupuytren's Contraction with Rheumatic Gout.

Col. C—— is the subject of Dupuytren's contraction affecting both hands. The thumbs are symmetrically affected; as also are the ring fingers. In addition, the left little finger and the right middle finger are involved. Col. C—— is a flute player, and has also suffered from rheumatic gout, which is in his family.

Dupuytren's Contraction in association with Gout.

Dr. F——, aged 66, was the subject of Dupuytren's contraction of the palmar fascia of twenty years' duration, the right hand being worse. The exceptional point was, that there was a bridle crossing from the thumb to the forefinger in both hands. In the left hand it was a strong one of a T shape, and much limited the movements of the thumb. In this hand there was but little drawing down of the ring finger; whilst in the other one there was but little in the thumb and much in the finger. Gout had been inherited through three generations, and Dr. F—— himself had had three attacks, and in his left ear was a single tophus. The urine was always clear and pale. For years he had been in the habit of carrying a walking-stick.

On the development of Dupuytren's Contraction during attacks of Rheumatism.

I have recently obtained a fact which has interested me very much in reference to the remote causes of this malady. It is one which also throws very important light upon the laws of hereditary transmission. A lady of about 30 came under my care, on account of eczema about the borders of.

her armpits. She was of fair skin and stout. Both her parents had suffered from gout, and she was herself dyspeptic and had had occasional twinges in her great toes. My attention was at once attracted to the condition of her hands; for both little fingers were drawn down towards the palm. There was induration of a band of fascia in front of them, and although the condition was by no means a typical form of Dupuytren's contraction, there could be no doubt that it was an approach to it. The left hand was more affected than the right. I was told that the condition had been left after a bad attack of rheumatic fever at the age of 14. Now although, as some of the preceding cases have proved, Dupuytren's contraction may be met with as a congenital condition, it is certainly when acquired extremely rare in youth, and almost unknown in the female sex. Yet here we have an instance of its development in a young girl. That rheumatic fever is frequently met with in the children of the gouty is an observation as old as Heberden; and in the present instance it is not, I think, going beyond the bounds of probability to suggest that the contraction of the fascia, which is usually a phenomenon of gout, was initiated during the turmoil of the so-called rheumatic fever.

It is of course possible that this patient had some congenital tendency to it, which became exaggerated during the acute rheumatism. Her condition was very closely similar to that of the cases which are congenital. It is scarcely necessary to ask attention to the strong support that this case gives to the theory that Dupuytren's contraction is generally in association with hereditary gout. In this connection the following case is of interest.

Symmetrical Contraction of the little fingers in a child who suffered severely from Rheumatism.

A child who was shown to me in the Great Ormond Street Hospital by Dr. Barlow, and who was the subject of very severe chronic rheumatism, had the little fingers drawn towards the palms much in the manner noticed in the case of congenital Dupuytren's contraction. There was, how-

ever, in her case no proof that the condition had been congenital. It was believed to have come on in connection with the rheumatic affection of the hands. Symmetrical contraction of the fascia in front of the little fingers is not, however, an ordinary result of a rheumatic attack.

Cases occurring in Women.

The exemption of women is by no means absolute.

In 1876 I had under my care Mrs. Sarah V——, aged 65, who was the subject of it, and in the same year Mrs. G——, aged 55, also presented an example of it. In the latter case there was, I know, gout in the family and in the patient. In the former, I have no note as to any inquiries.

Cases in Young Adults.

A gentleman named W——, who was under my care in 1894, and in whom it had begun at the early age of 25, told me that his mother had the same, and added that she was very gouty. In this instance, although the ring and little fingers were affected, the most noticeable contraction was between the thumb and index.

Mr. Adams has told me that he has notes of only two cases in men under twenty.

ON ERUPTIONS WHICH RECUR PERIODICALLY (CATARRHAL ERUPTIONS).

(*Continued from Vol. V., page* 215.*)

At page 203 of Vol. V. of ARCHIVES will be found a paper on Catarrhal Eruptions. In this paper I attempted a definition of the group, and gave the particulars of eight cases which appeared to belong to it. The suggestion was that there are certain forms of eruption which tend to recur periodically after longer or shorter intervals, which are of limited duration, and disappear spontaneously; and that it is not improbable that in some instances they depend upon a similar disturbance of nervous balance to that which produces an ordinary cold in the head. In some instances, those who are liable to these eruptions will allege that they occur in connection with a chill, and seem to substitute an ordinary nasal catarrh. I may admit, however, that this is not a very frequent experience, and that in the majority of instances it is the periodical recurrence and the spontaneous disappearance, taken together with the absence of any other suggested cause, which seem to ally these eruptions to catarrhal maladies. Without insisting too much on the word "catarrhal," we may, I think, at any rate assume that these eruptions are of neurotic origin. They appear to be explosions to which the patient becomes liable after a period of immunity more or less long. Some of the eruptions to which I refer would be claimed by dermatologists as Erythema multiforme, Herpes iris, Cheiro-

* I find that I have since, under the heading of recurring maladies not restricted to the skin, given several cases which really belong to this category. See ARCHIVES, Vol. VII., pages 169 and 227. The present paper may be regarded as the fourth dealing with this topic.

pompholyx, or even Erythema nodosum. It is not, however, so much upon the anatomical character of the lesions as upon their periodicity, apparent causelessness, and tendency to spontaneous involution that I wish to insist as being the characters of the group.

The cases narrated in the former portion of my paper were eight in number, of which 1, 2, and 3 were perhaps the most conclusively associated with catarrhal tendencies. In case 3 especially the association was most definite. In all the cases, with one exception, numerous relapses had occurred, and in case 6 the liability had extended over thirty years. To these I have now to add several others.

CASE IX.—*Two attacks at nine months' interval of an erythematous eruption, apparently of a catarrhal character.*

In June, 1870, I saw a woman named Mrs. R——, aged 42, who had had two attacks of a peculiar eruption. In each instance it had disappeared spontaneously after about a week's duration, and on each occasion she had had a sore throat at the same time and thought she had caught cold. Her first attack was in December, 1869. The eruption consisted of scattered spots on the face and arms. They were of a dusky copper tint, and at first sight suggested syphilis. There was, however, no history whatever to support that suspicion, and their complete disappearance in ten days put it out of the question.

CASE X.—*Recurring attacks of a vesicating eruption on hands and feet, during four years.*

A young woman named H——, who was under my care at the Hospital for Diseases of the Skin many years ago, had been liable for four years to attacks of a vesicating eruption on her hands. She said that it had occurred twice every summer, but never in winter. The attacks varied in duration from a fortnight to two months, and never affected any other parts than the hands and feet. Before the eruption appeared she usually felt dull and low spirited, and when the eruption was coming out her hands would feel hot and

uncomfortable. The vesicles would usually break and dry up, but never formed scabs. During an attack in which I saw her at the end of one August, her fingers were covered with small vesicles, as if they had been lightly scalded. In the palm of the hand there were some much larger than others. They were quite transparent, and as big as large peas. There was severe burning, but no itching. Her hands were usually very hot and accustomed to perspire much. She was a fair-complexioned woman, aged 29, pale and in tolerably good health.

CASE XI.—*Recurring Vesications on hands.*

In another case, which was very like the preceding, with the difference that the eruption was seen in a first attack, the patient was a young woman of 26. The hands only were affected, and the attack having lasted about six weeks, ended in general desquamation.

CASE XII.—*Recurring attacks of Erysipelatoid Erythema of the face—Quinine as a possible cause.*

Quinine is one of the drugs to which suspicion must always attach in these cases of recurring erythema. Lady S—— was under my care three or four times during ten years for recurring attacks of inflammation of the face, much resembling nettlerash. On one occasion the limbs were affected and the face exempt. She was about 40 years of age, and in earlier life had never experienced any liability to skin disease. In every instance the attack on the face had begun on the ears by flushing and tingling, quickly followed by redness and swelling of the eyelids, nose, and cheeks. One of the worst attacks appeared to have been evoked by the local use of arnica, and others had apparently followed the internal administration of quinine.

A very similar case is the following.

CASE XIII.—*Recurring attacks of Erysipelatoid Erythema of face.*

Mrs. W. E. F—— was sent to me in March, 1891, on account of what was designated an erysipelatous form of

erythema with chronic irritability of the eyelids; and she brought with her a letter informing me that she had taken the solution of the perchloride of mercury for a long time, and that it was the only thing that seemed to relieve her. Mrs. F—— was 70 years of age; but looked younger. She had enjoyed good health during life. She had suffered during three years, and chiefly in winter, from attacks of inflammation of the face and hands, which I was assured every one would have called erysipelas, had it not been that there was no material rise of temperature. Her first attack had occurred at Torquay after exposure to the wind, and she was laid up in bed for a few days. Peeling of the hands and face usually followed the attacks. She was liable to flush very easily and often felt very chilly, but dared not go near the fire.

CASE XIV.—*Recurrent attacks of a papulate Erythema during thirty-seven years.*

An unmarried woman, aged 53 (B——), afforded an example of an eruption which had been repeatedly recurrent, and which in the main conformed to the type of erythema nodosum. She described her first attack as having occurred at the age of 16. Her legs only were affected, and the eruption consisted of red patches, with swelling and much pain. She had also a good deal of rheumatic aching. After that she had other attacks at intervals of a few years through the rest of her life. It was in November, 1877, when, as stated, she was 53 years of age, that she came with a copious eruption on her arms and legs. In former attacks she had never had it on the arms, nor on the lower extremities ever above the knees, but now there were spots on the thighs and one or two on the face. This was the most severe attack she had ever had. The eruption consisted of circumscribed lumps and patches of a dusky red colour, some of them showing a tendency to vesicate. It was most abundant on the backs of the limbs, where it was very copious. The patient stated that she always felt languid and weak before and during the attacks, and was relieved after them

The eruption, during the attack which I watched, took a month to fade and left dusky stains.

It will be seen that in this case the eruption differed a little from erythema nodosum in that it occurred chiefly on the backs of the limbs. It was also papular rather than nodose. The patient described it as not having been in the first instance attended by lumps. The case does not, however, range quite with other cases of recurring erythema multiforme, inasmuch as the legs and not the hands were the parts affected.

CASE XV.—*Recurring attacks of Cheiro-pompholyx.*

Dr. Clouting, of Thetford, brought to me in March, 1879, a gentleman who suffered from this form of eruption. He was a fair and florid man, under middle age. It was five years since his first attack, and during the two following years he had attacks every six months, but recently they had been more frequent. His first attack was the most severe of all, the hands being completely covered. His earlier attacks had, however, been more definitely transitory than his later ones, some of which had threatened to persist. The hands and feet usually suffered together, but the hands much the more severely. Some abortive blisters were always observed, but they never developed into large bullæ. Their tendency was to dry up in the middle and spread at the edge. There was never any itching, but a burning sensation as if the parts had been scalded. He was engaged in outdoor occupations, and he thought a little over-exertion had frequently produced an attack. He also thought that constipation sometimes caused it.

I heard from Dr. Clouting in March, 1884, *i.e.*, five years after the date of the above notes, that Mr. C—— was still liable to his attacks at intervals, but that they were mild and usually connected with over-exertion.

CASE XVI.—*An eruption resembling Erythema Multiforme two years after Syphilis.*

In the case of a policeman named G——, aged 32, we had a curious eruption on the backs of the hands, on the penis,

face, elbows, and knees. The condition was one of erythema papulatum, with tendency to vesicate. In the palms of the hands the skin peeled and left patches much like those frequently seen in syphilis. The man admitted that two years ago he had had a chancre, and said that six months after it an eruption very similar to the present one had appeared and had lasted three weeks. The present eruption had developed suddenly. We ascertained that he had not been taking iodide of potash.

CASE XVII.—*Recurring Vesicating Eruption on hands and feet.*

A gentleman named P——, whom I saw in September, 1880, had had many attacks of a vesicating eruption on his hands. It usually avoided the palms, and affected the sides of the hands and clefts of the fingers. On one occasion it attacked his feet, and here also it avoided the sole and produced a border of vesications around the edges of the foot. It was well in a fortnight, and did not confine him to bed. Mr. P—— knew of no special cause for his attacks. He was dyspeptic, and sometimes had phosphates in his urine. This did not, however, interfere much with his general health. He said that he was very catarrhal, and used to catch cold easily, both summer and winter. He attributed his dyspepsia to his vocation as a tea-taster, and said that what most relieved his hands during the attacks was to bathe them in very hot water. He was about 35 years of age.

CASE XVIII.—*Erythema Multiforme on the backs of the hands of a young man—Annual attacks at first becoming more frequent during seven years—Excellent health and no other liabilities.*

A gentleman named Mr. H. T—— offered us a very good example of the malady. He was good enough to attend at one of my Demonstrations in order to allow his hands to be seen. He was 25 years of age, and had been a total abstainer all his life. His liability to erythema multiforme had extended

over seven years, during the whole of which time he had in other respects enjoyed very good health. At first his attacks used to occur usually in summer, and only once a year. Very often he noticed that he had them during a holiday, probably in connection with increased exposure to sun and air. Latterly they had usually occurred twice a year, and at somewhat irregular intervals. One attack had occurred in winter, and that for which he consulted me was in spring. He assured me that he felt perfectly well, both before, during and after the attacks. His chief annoyance was the appearance of the eruption on his hands. The attack in which I saw him was a very well-characterised one, the backs of the hands being covered with florid or salmon-tinted blotches, some of them almost as large as a shilling. He had no eruption elsewhere, but his ears would become red and itch when the spots were out on his hands. His attacks, he said, usually lasted some weeks, and left stains which remained yet longer. The attack, during which I saw him, had commenced on a Sunday, the backs of the hands being irritable and the ears itching and hot. Next day a few spots had appeared on the hands. On the fourth day some of the smaller spots has coalesced, and there were slight appearances of vesications. On the seventh day the light redness changed to a dusky tint, and after this the blotches flattened out and ceased to vesicate.

CASE XIX.—*A case of Erythema Nodosum, in which the eruption assumed the characters of thrombotic purpura and also vesicated.*

A boy, aged 11, named S——, who was under my care at the Hospital for Skin Diseases in October, 1875, went through a transitory attack attended by a thrombotic eruption and œdema. He had been attending for ringworm, and had been taking small doses of arsenic, but these had been left off ten days before the eruption showed itself. He was in fair general health, and no special cause for the eruption could be assigned. The eruption affected his legs, and was almost symmetrical. It consisted of patches of various sizes

from a pea to a half-crown, which were dusky and livid like erythema nodosum, but not in the least raised. Their colour was not modified by stretching the skin. They were abruptly margined, but, excepting the small ones, none were round, their edges being irregular. A few of them had developed vesications as large as sixpences. The legs were œdematous. On the forearms were groups of small spots of a similar kind, and just over the upper part of each ulna there was a considerable œdematous swelling. So absolutely absent were all signs of congestion over these œdematous swellings that at first we doubted whether they were only peculiarities of contour. On pressure, however, they pitted deeply. It might have been that they were in part due to pressure, as the boy, in his vocation as a newspaper vender, was in the habit of carrying a bundle of papers first under one arm, then under the other. The spots on the forearms were not erythematous, but looked like little bites or scratches. I admitted this boy into the London Hospital in order to give a Clinical Lecture on his case. When the day arrived, however, the eruption had wholly vanished and scarcely any staining remained. Its whole duration was little more than a week.

The arrangement of the eruption in this case, and the tendency to œdema which accompanied it, seemed to place it in the group of erythema nodosum. In its anatomical characters, however, it differed very considerably, being of a purpuric character. There were no rheumatic complications.

The following case, which I observed some years later, was one in which almost exactly similar conditions were displayed, whilst we had a history of repeated recurrences at short intervals.

CASE XX.—*Liability during three years to very frequent recurrences of a thrombotic eruption on the limbs, attended by much soft œdema—No special cause assignable.*

The following case appears to be an example of almost persistent liability to thrombotic purpura. Its subject was

a tall, spare woman of sixty, living at Norwich. I saw her first in October, 1877, when she had come up to town for advice. She showed me her arms and legs, which were mottled over with purpuric stains and in parts œdematous. In the legs there was a thick roll of œdema just above her boot-tops, and in the forearms just below the elbows there were large ill-defined areas which were puffy and swollen. She also directed my attention to two puffy swellings, ill-defined and by no means conspicuous, on her forehead. The purpura when I saw her was fading, and had left the skin, over large areas, stained and dusky, with, however, a few definite spots still present. The œdema was so very soft that at first I almost doubted whether any was present, but on careful inspection its existence in considerable degree became certain.

Our patient stated that in early life she had had rheumatic fever, but after that had always enjoyed excellent health until three years ago she began to suffer from her present complaint. The first outbreak was rather severe. Suddenly her arms, thighs, and legs became covered with scarlet spots, and swelled much. There was no itching, but some pricking and tingling. In three days the spots were fading. She had felt languid and ailing for a few days before this eruption. Her second attack was a week later, and the third a month after that. Since then she had been liable to an attack every two or three weeks. The trunk had never been affected, the eruption being confined to the limbs and head. On the limbs the eruption had affected the hips and shoulders, but never the trunk. On the face only puffy swellings had occurred, no purpuric spots. The eyelids would swell up.

Mrs. B—— said that she always knew when the attacks were coming, as she felt languid and unable to work. The parts would tingle and swell before the spots appeared. There was much burning heat and great swelling. As the eruption was going, the skin looked exactly as if it had been bruised. She was not catarrhal, and never liable to colds. Is not liable to constipation, and is not dyspeptic.

CASE XXI.—*Symmetrical and general Erythema Papulatum, with perhaps extravasations or thromboses, in a healthy old woman—Sudden outbreak after a slight chill.*

[*Transcript of notes taken at the time.*]

Mrs. C——, æt. 67 (September 27, 1875). She is a hale, vigorous old woman; she never had any skin disease before. Has never been laid up by rheumatism, but occasionally has had slight pains in some joints. On Thursday (September 22nd) she moved from her former room to a new house in a neighbouring street; it was a hard day's work, and at night she went to bed without taking anything, and without a fire; she was very tired; slept soundly, but woke up chilly; had not had so much bed-clothing as usual because her blankets were not unpacked.

On evening of next day (Friday) she noticed a few spots on her arms; next morning (Saturday) they were very numerous, and by that evening or Sunday morning she thinks they were fully out. She had felt a little chilly ever since the Thursday evening, but as she often and easily takes cold this was not anything unusual with her. In other respects she felt as well as usual. There appears to have been no dietetic cause, such as shell-fish. She never had nettlerash.

She came to the Skin Hospital on Monday (27th), and the eruption was then as copious as now, but of somewhat brighter red colour. It is to-day becoming decidedly purplish. There has throughout been no itching. There has been slight swelling of feet from walking during the last few weeks, but no spots came out until the above date, and at no time has the swelling been nearly so much as now.

September 29, 1875.—The eruption is remarkable for its all but universal distribution. It affects the face, neck, and extremities; the fingers to the very ends, and the palms and soles. It consists of papules varying in size from a shot to a split pea, or a little larger; they are but little raised, and although perceptible to the touch as thickening, do not *roughen* the skin. At present (fifth day) they are dusky, having lost the bright tint which they had earlier. They occur on the face, but not so freely as on other parts, and cease gradually on the forehead. There is a good deal of œdema of the lower extremities. The colour of the spots cannot be made to disappear by stretching, although it is considerably diminished. Their margins are ill-defined, and they do not look like extravasations; nor can individual vessels be traced running from them as in some forms of thrombosis. In many parts the spots become confluent, and large, very irregular patches are produced. Where the patches can be made to lose their red colour entirely by stretching, slight brown discoloration usually remains. It is clear that the eruption is chiefly due to congestion, and very probably this congestion occurs in the vascular papillæ. Whether any true extravasation has taken place at any parts is doubtful; probably there is some blood-staining by imbibition, and very probably there is thrombosis in some of the patches.

CASE XXII.—*Recurring attacks of Erythematous patches on the face in association with Raynaud's Phenomena in hands—Query, Catarrhal.*

Mrs. C——, a lady of thirty-five, fair, florid, and in good health, was liable to an eruption on her face which gave her much trouble. It had recurred about once every three months, and she counted six attacks. With the attacks she was accustomed to have headaches, and to feel ill and feverish. The eruption consisted in red blotches as large as florins, which were attended by intense burning and pricking. No pimples, pustules, or vesicles ever formed. The erythematous blotches were all symmetrical, and partly because they were so disfiguring, she was compelled to keep her own room. She had usually attributed her attacks to some form of chill. On the last occasion after playing at tennis, she had shivered whilst sitting in the open air. On the next day her face was covered with blotches. Although apparently in good health, her circulation was easily disturbed. Thus her hands would become white and the nails almost black after a bath, and the nose was sometimes affected in a similar manner. Her lips, she said, were sometimes blue. She had never had chilblains.

CASE. XXIII.—*Catarrhal affections of the Skin.*

Mrs. R——, aged 48, the mother of seven children, was sent to me by Dr. Brooke, of Manchester, in April, 1889. She was the subject of a sort of eczema, producing appearances much like pemphigus foliaceus. Her skin had always been rough, and four years previously she had the first attack of her present malady. It was always well during the summer, but relapsed in the spring. The attack for which I saw her was of three months' duration. Her face was patchy, with abrasions as if there had been bullæ. I could find none of the latter; but she described little blebs, not bigger than peas, as having been present. The eruption seemed to be spreading down her trunk. The mouth was not inflamed in the least, and the tongue was pale and perfectly clean. During some attacks the lips had peeled.

All ointments seemed to irritate, and she thought that she always got better when she let the skin get dry and applied nothing. The eruption had been very pruriginous. For three years running, before the skin was affected, she had to keep her room for a month at a time with a bronchitic cold. The eruption seemed to have taken the place of the colds. The last attack had begun after sitting in a draught at the dinner-table, and was associated with swelling and stiffening of the eyelids.

Mrs. R—— had also suffered from "blackness of the hands" in the afternoons and evenings. It disappeared when she got warm in bed, and was probably of the same nature as Raynaud's phenomena. The eyes had also been inflamed. There was a history of gout in the family.

NOTES ON YAWS.

(*Continued from p.* 123.)

Our knowledge of Yaws advances with great rapidity, and we may reasonably expect that satisfactory conclusions as to its nature and treatment will soon be attained. When five years ago Dr. Numa Rat published his Government Report on the malady, but few English surgeons knew more than its name, and very erroneous statements as to its characters and prevalence were to be found in our text-books. A little prior to that date I had had executed, from some photographs which had been sent me, some coloured representations of the appearances which the disease presents. When I published these, I stated that, so far as I knew, they were the only delineations of the malady extant in England, and that none of our Atlases, English or foreign, contained any.* Since then I have been supplied with photographs and coloured portraits by many surgeons resident in countries where the disease prevails, and the Clinical Museum now contains a very large number. An important and liberally illustrated Report by Dr. Alford Nichols has also been published by our Colonial Office. Nor has the subject been wholly neglected by foreign surgeons, although they, for obvious reasons, are likely to have had it less frequently and less urgently brought under notice. By Dr. Breda, of Padua, a report has been published in the "Archiv für Dermatologie und Syphilis," 1895, which contains illustrations of a very instructive character. From the published essays named I have collected all the plates, and these, added to those

* The plate illustrating Peruvian Framboesia, published in the New Sydenham Society's Atlas, represented a disease quite distinct from true Yaws.

PLATE CLX.

FRAMBŒSIA IN AN ENGLISHMAN.

This Plate, copied from a photograph, represents a well-characterised example of Frambœsia or Yaws Eruption in an Englishman. Want of space has necessitated the omission of the notes of the case. They will be given in the next number of 'Archives.'

PLATE CLX.

[illegible] ENGLISHMAN

[illegible] from a photograph, represents a well [illegible] of [illegible] or Yaws Eruption in an [illegible] the [illegible]

West, Newman lith.

given from private sources, enable me to assert that on the walls and in the portfolios of the Clinical Museum may now be found sufficient material to enable any one to form a very clear conception of the features presented by this disputable malady. The collection, coming as it does from various sources, is of especial value as proving beyond dispute that the malady known as Parangi in Ceylon, Coko in Fiji, and Yaws or Framboesia at the Cape and in the West Indies and elsewhere, is really one and the same disease.

The most valuable of all the gifts to my Museum comes from Dr. Kynsey, of Ceylon, and consists of a series of beautifully executed water-colour drawings showing Yaws in its various stages, another series from the microscope, and a manuscript essay on the malady. This volume was exhibited in the temporary museum of the International Congress, and has since, as requested by the generous donor, been deposited in the collection referred to. The only drawback to my gratification in receiving such a splendid gift is that I may be compelled to differ from Dr. Kynsey's opinions, and may be obliged to use the facts, which he has with so much skill and labour supplied to me, to prove, what he is still concerned to deny, that Yaws is really nothing but syphilis. I would earnestly invite any one interested in the matter to look through Dr. Kynsey's volume and form his own conclusions.

Dr. Kynsey summarises his creed as follows:

1. "I consider Framboesia and Syphilis absolutely distinct diseases.
2. "I have no doubt that Framboesia, the Button Scurvy, the Sivvens of Scotland, and probably the Radesyge of Norway and Sweden, are similar.
3. "That the ulcerations after the eruptive stage are Lupus."

The reply to these is obviously that Button Scurvy, Sivvens, and Radesyge are now generally acknowledged to have been names for Syphilis, and that if the serpiginous ulcers which follow Framboesia are a kind of "lupus," so also are those which follow syphilis.

Dr. Kynsey gives the following descriptive definition of the malady:—

Parangi (= Yaws) may be defined as a disease *sui generis* marked by an ill-defined period of incubation, followed by vague premonitory symptoms such as slight fever, which may be accidental; muscular and articular pains, which may be severe, scarcely perceptible, or absent; and the evolution of successive crops of a characteristic eruption which runs a definite course. It is propagated by contagion, the virus entering through an abrasion or sore. It attacks irrespective of age, race, or sex. It yields readily to early and appropriate treatment. As a rule it does not interfere with the general health, and an individual may be attacked more than once. "Although the duration of the above-defined disease may be as short as three months, it is usually much longer. The cases may be described as acute or sub-acute, the eruption coming out in sucessive crops. Each crop may consist of one or two, or of many tubercles. After a variable period, which may be months or years (but in most cases not at all), Frambœsia is followed by chronic ulcerations, the nature of which is not settled."

An argument of some little interest may be derived from the name given to the disease by the natives in Ceylon. They call it Parangi, which means the foreign disease, and they credit the Portuguese with having introduced it. Now this is precisely the name which syphilis has in various forms received in other countries. The Portuguese are known to have suffered much from syphilis, but it is not recorded that they were ever liable to Yaws or any similar malady.

Great importance is to be attached to a narrative of some extensive inoculation experiments performed in Java by Dr. M. Charlouis, in 1880. Four Yaws patients were selected, and from their sores thirty-two prisoners, all healthy and free from disease, were inoculated. Of these thirty-two only four failed. "After the lapse of fourteen days papules made their appearance at the seat of inoculation, each surrounded by a red halo. Six days later pustules slowly developed, crusts subsequently formed, on the removal

of which the characteristic fungus was seen. In most cases the general symptoms of Frambœsia showed themselves three to five months afterwards. In all the cases the patients complained of painful glandular swellings, the same as occurs after the inoculation of syphilis." Thus far, then, the results were almost exactly like those of syphilis, and the author mentions a series of seven features in which the two diseases are alike. He finally, however, concludes against their identity on the strength chiefly of two cases. One of these was an experimental inoculation with syphilis of a patient who had Frambœsia, and in whom a chancre and an eruption followed; the other a case in which a Frambœsia patient acquired syphilis and had a characteristic eruption. These facts, it must be admitted, would be strong did they not stand almost alone, and were it not that it is well established that syphilis may occur twice and occasionally after very short intervals. Dr. Charlouis states distinctly that the several stages of Yaws are curable by the same remedies as would be used for syphilis.

Yaws in Venezuela.

The subjoined abstract I take from the *British Medical Journal.* "La Buba" is a synonym for Yaws.

"A paper on 'The Clinical Study of the Disease Known as "La Buba"' was read by Drs. Luis Razetti and N. Guardia, jun., of the University of Caracas. They described La Buba as a contagious, inoculable disease, endemic in some tropical regions, and characterised by a vesiculo-pustular eruption, followed by fungoid ulcerations. There was no fever unless there were a secondary infection; no prodromata. The eruption began as a vesicle which, after a period more or less long, became an ulcer, called the mother buba. This mother buba was not permanent. It might disappear before the secondary phenomena presented themselves, or might persist during the whole course of the disease or even longer. The mother buba presented itself only in portions of the surfaces of the body not covered by dress. The secondary manifestations consisted of a general eruption of ulcerating pustules. These become fungoid, and covered with a yellowish crust. These pustules were the seat of pain and itching. The mother buba alone presented an indurated base, and was followed by a scar; whilst the other ulcers left only spots that subsequently disappeared. The duration

was from nine to twenty-four months. A solution of continuity of the skin was indispensable for the contagion. This might be produced by the bite of insects. The bulbs was then an infectious disease, and must have for its cause a micro-organism not yet isolated. The disease was not a syphilitic manifestation. There was no hereditary tendency. The bulbs could not be confounded with any other disease. The only treatment thus far employed consisted of mercury, iodide of potassium, and antiseptic of the skin. Prophylaxis was of the greatest importance."

The following occurs in a report given in the *Lancet*, August, 1883, page 337, of an Athenian Medical Congress:—

"An alleged syphilitic cachexia, the symptoms of which are not described, is endemic in some parts of Greece. The nature of this affection, which is said to have a congener in Norway called Radesyge, and in Scotland termed Sivvens, was discussed by Albanakis of Calamas."

Reputed absence of Syphilis in the Fiji Islands, and prevalence of Yaws.

The history of Yaws in the Fiji Islands may also throw some light on the question of its nature. I was told some years ago by a surgeon from Fiji, that there Yaws almost wholly took the place of syphilis, that there was much Yaws and very little syphilis. A most interesting letter with which I have just been favoured by the Government Medical Officer of Fiji, and which I subjoin, goes even further than this, and alleges that syphilis is there unknown. The writer believes that Yaws has prevailed in these Islands from time immemorial, and states that parents wish their children to go through it young. Now the Fiji Islands are not only beautiful, but very productive of fruit, &c., and they have been known to Europeans since the time of Tasman, who discovered them in 1648. The suggestions which occur in reference to the statements made in my correspondent's letter are (1) That it is very improbable that the Fijians should during two centuries have escaped the introduction of syphilis. (2) That it is also improbable that they should from time immemorial have had in their midst a specific contagious disease so exactly like syphilis, and from which most other aboriginal populations appear to have been free.

If Yaws be syphilis modified by race, it is easy to explain

the supposed absence of the latter from Fiji, for it has all the time had another name. There are many questions of great interest to which answers are needed before we can arrive at satisfactory conclusions on these points. It is clear that we have in Fiji a field for investigation of great value. In the asserted entire absence of syphilis, are the lupoid tertiaries of Yaws as common as in other countries? Is inheritance ever observed? Is the disease ever communicated to whites?

GOVERNMENT STATION, REWA, FIJI.

DEAR SIR,—I have read with interest your articles on Yaws in the January and April numbers of the ARCHIVES, and my interest in the subject must be my excuse for writing you.

As probably you read in Dr. H. A. Alford Nicholls' Report on Yaws in the West Indies, a disease locally here known as "Coko" is common among Fijians, and that disease is identical, I believe, with the Frambœsia of the West Indies. A few facts concerning Coko and its sufferers may therefore be of interest to you.

Fijians, as far as is known, have never had Syphilis. Dr. Corney, the chief medical officer of this colony, stated so to me, and during the four years that I have been in the service I have never come across a case. My neighbour here, Mr. Carew, who has been commissioner and magistrate since annexation, confirms this.

Fijians are but of recent civilisation. Thirty years ago they knew neither how to write nor how to reckon time; consequently it is difficult to get an account of how their ancestors came here; but the old chiefs all maintain that Coko is, as they term it, "a disease of this land" (not introduced), and that all Fijians have always had it in their infancy—not only those of to-day, but their grandfathers, great-grandfathers, and probably, too, the ancestral spirits who constituted their gods until Christianity was introduced. If a child does not develope Coko they consider it unhealthy; they prefer the children to indulge in it during the first three years of their lives, for after that, if they develope it, they usually have it in a severer form than is agreeable.

As a rule they object to treatment, and frequently the eruption lasts for eight or nine months, or longer, and a certain number of the children undoubtedly die from complications of the disease, but the number of infant deaths attributable solely to Coko is not, I believe, high. Still, the disease doubtlessly predisposes to marasmus in certain cases.

As sequelæ, a form of strumous ulceration of the fauces and palate, as well as of the vagina, occurs. This is known among the natives as "kanailorna" (?). They do not, however, believe in the connection between the two complaints. The only sequela they acknowledge is the "Saki"—their name for the granulomata that occur about the heels and

soles of the feet which is termed "tubboes," or "crabs," in Dr. Nicholls' work. These Fijians also call "the after coming Coko." Some months ago I sent to the *Lancet* an account of two cases of Coko which occurred in hospital here. The patients were East Indian immigrants, catching the complaint from Fijian infected children—by fly inoculation probably, as they had ulcers of their ankles and the mother sore appeared at these spots. I sent photographs with the account for publication in the *Lancet*.

As to the characters of the eruption, they tally with those given by Numa Rat, Nicholls, Pierez, and Prout in their respective papers. The primary sore at the point of inoculation is not universal, though the rule; still, I have inoculated fowls, and in one case a general eruption occurred, but no "mother sore." At first the eruption is papular, papules about the size of a pin's head; later on papules become tubercles, some coalesce; the apex breaks; serum of a yellow colour exudes; crusts form, beneath which the more or less raspberry spots can be seen. The rash is not confined to any one part of the body; all parts are or may be affected, especially such as at anus or mouth, and there I will confess they remind one of condylomata. The tubercles do not appear all at the same time, but come out in crops. The general tendency is towards recovery. The eruption is frequently ushered in by a rise in temperature. In some cases the tubercles develope into ulcers, which are of a callous and chronic form.

As to sex, both seem to be affected alike. Serum from Coko produces a cloudiness in from two to three days in gelatine. Colonies of micrococci develope which when injected into fowls produce typical eruptions. As to treatment, strong nitrate of mercury ointment is useful, and iodide of potassium and syrup ferri iod. answers well internally. Mercury I have not found to be of much use. In chronic cases arsenic answers well.

Believe me to remain,

Faithfully yours,

District Medical Officer, Rewa, Fiji.

Dr. Arthur Powell's Report.

A manuscript paper on Yaws as observed in the East Indies, has been lent to me by Dr. Arthur Powell, who has courteously given me permission to publish extracts from it. The essay is illustrated by photographs and drawings, all showing exactly similar appearances to those given by other authorities. Some of the photographs do not show a strictly frambœsial eruption, but rather serpiginous patches of greater or less size with granulating and crusted borders. These are, so far as I am a judge, exactly like those not unfrequently seen in syphilis.

Dr. Powell traverses some of Dr. Numa Rat's statements, and is inclined to suspect that some of them were based on the observation of cases in which syphilis was really present. He denies Dr. Rat's statement that lesions of a tertiary class follow Yaws, and says repeatedly that he has never seen any. In this, however, his experience differs from that of many other observers beside Dr. Rat.

By far the most important statement in Dr. Powell's essay is that in which he describes two cases in which patients, who were actually suffering from Yaws, contracted syphilis, and showed the usual phenomena.

Dr. Powell gives categorical answers to the questions which I appended to Dr. Rat's essay (see ARCHIVES, Vol. VII., p. 112).

I. He cites many references to the occurrence of Yaws in Europeans, but admits that instances of it are rare.

II. He thinks that syphilis and Yaws do not show any tendency to prevail in inverse ratio to each other.

III. He believes inherited syphilis to be as frequent in Yaws districts as in others.

IV. He admits that he has never known any child the subject of inherited syphilis to contract Yaws.

V. He does not think the differential diagnosis between Yaws and syphilis difficult. (Yet, as has been said, he thinks that an observer so able as Dr. Numa Rat did describe as Yaws what was really syphilis.)

VI. He mentions two cases in which women who had recently had Yaws bore children showing no signs of syphilis, and two others in which the putative father had had Yaws. (No details are given as to how long the children were under observation.)

VII. He does not think that syphilis, when occurring in the dark races, shows any peculiarities.

VIII. & IX. He has seen sexual chancres in coloured persons as hard as any met with in Europe, but admits that they are not often shown. He believes that bullet buboes always attend hard chancres.

X. He writes: "I have never seen tertiary symptoms in Yaws;" and to the question as to whether the two cases

described by Rat may not have been instances of the co-existence of syphilis and Yaws, says, "Emphatically Yes."

XII. He asserts that the interval between Yaws and syphilis may be only a few months. (For details see Transactions of Indian Medical Congress.)

XIII. As the result of careful examination of the throat in the secondary stage of Yaws, he states that with two exceptions he has never found it affected.

Drs. Wallbridge and Daniels' Report on Yaws.

Much important information respecting Yaws, especially as seen in the Fiji Islands, is to be found in a report just made to the Colonial Government by Drs. Wallbridge and Daniels. The first of these observers has lived in British Guiana, and the latter in Fiji. From their report I will make a few extracts. The hypothesis of contagion by flies is adopted as explaining the origin of many cases. In reference to the resemblance of Yaws to syphilis Dr. Daniels writes:

"The relation between Yaws and syphilis is of great importance, as, whilst it is certain that they are closely analogous, it is equally certain that they are separate and distinct diseases. No doubt most people on seeing their first case of Yaws would think as I did, that 'this must be a syphilide,' though I never saw one like it. When, however, instead of one, you meet with hundreds all alike, its recognition as a distinct disease is easy, and once the characteristic appearance is known, diagnosis is unmistakable in most cases."

As to the occurrence of syphilis and Yaws in the same person at different times, the following important evidence is offered:

"A fair number of such cases are recorded, and in all of these *the syphilis preceded the Yaws.*"

This, of course, does not relate to Fiji. As to the latter, Dr. Daniels writes:

"I only heard of one such case in Fiji. In this, a European, the syphilis preceded the Yaws, but the latter disappeared during a voyage to England, and on return of the patient to Fiji he had a well-marked tertiary syphilitic eruption."

This reads very much as if the so-called Yaws was only a stage of the syphilis developed into peculiarity by a tropical climate.

As regards the extreme rarity of syphilis in Fiji, if not its entire absence, Dr. Daniels tells us—

"Though not a licentious race, the Fijians have had both from Europeans and East Indians abundant opportunities of acquiring syphilis. As a speculation it was frequently suggested that the Yaws were to some extent a protection."

This statement is of the greatest possible interest, for the theory of Dr. Daniels is that the Fijian population has been from time immemorial saturated with Yaws, the whole population having suffered from it.* If such a population be really immune from syphilis the inference is obvious. Dr. Daniels goes on to add:

"In this connection it is especially noteworthy that in all the conclusive cases of persons being attacked with both syphilis and Yaws, the syphilis *preceded* the Yaws; though as the latter is usually acquired in childhood, we should expect to find Yaws precede syphilis in the larger proportion of cases."

This again supports the inference already arrived at, that Yaws does protect in some measure from syphilis, and further that syphilis may produce the appearances known as Yaws.

Dr. Wallbridge, speaking of British Guiana, says that the risk of contracting Yaws depends wholly upon the degree of carelessness with which exposure to contagion is encountered, and not upon social condition or general health, nor even upon race. "The most careless suffer most." As to race he adds:

"In British Guiana the disease has always been looked upon as

* The Fiji Islands are sixty in number, but two only of them are of important size. They have a population of 133,000. I find a mention of Coko, from a non-professional source, which is of some interest, since it supports the belief that Yaws, uncomplicated by syphilis, has a tertiary stage, of which bone disease is one of the commoner symptoms. The writer (Coutts Trotter) of the article "Fiji" in the "Encyclopædia Brittanica" says briefly, "The natives have a bad skin disease, Choko, *affecting also the bones*, from which few escape; but it is said to be avoidable by a sounder hygiene."

essentially a negro disease, and cases among the East Indians, who until recently have held themselves aloof from the negroes, have been rare, and the disease is still more rare among the white races."

As to the so-called tertiary symptoms in Yaws, we have—

"There are a series of pseudo-syphilitic phenomena met with in the natives (of Fiji) thought by some to have a connection with Yaws."

This is a very serious admission respecting islands in which Dr. Daniels strongly asserts that syphilis is unknown.

Dr. Poonen's experience of Yaws in India.

To Dr. E. Poonen, of the Trandrum Hospital, Travancore, I am indebted for some interesting items of information as to Parangi in India. He told me that the word itself means "Portuguese," or "the Portuguese disease," and that it is equally in use both in Ceylon and Southern India. He had chiefly seen the disease as a sort of family epidemic, very contagious, and often affecting all the members of a family. He said that in most of its symptoms it closely resembled syphilis, and added that it was easily cured by mercury and iodide of potassium. He was accustomed invariably to use one or other of these drugs. Although fully admitting that in most features there was nothing to distinguish it from syphilis, he yet thought it was not, like the latter, followed by disease of bones. I suggested that if bone disease did occur the case would probably be called syphilis, which he admitted. There is, however, another possible explanation of the comparative absence of bone mischief in Dr. Poonen's practice, in that he had been in the habit of using specifics. Nodes are rare in syphilis if adequate treatment be adopted.

THE NERVOUS SYSTEM.

No. LXXXV.—*Paralysis of the Extensor Communis Digitorum in one Arm of a healthy young man —No assignable cause.*

A gentleman, aged 25, who consulted me about another matter, disclosed a very exceptional condition of paralysis of a single muscle. The affected muscle was the common extensor of the fingers of his right hand. There was a deep hollow on the back of his forearm, and he was quite unable to extend his middle, ring, and little fingers. The extensor indicis was perfect. When the fingers were held by the other hand in extension he could effect an additional extension by means of the small muscles, but he could not demonstrate any power in the latter when the hand was unsupported. In some movements his hand would drop helplessly to the ulnar side at the wrist. There was no defect of sensation in any tract of skin nor any lesions of nutrition.

The history which Mr. F—— gave of it was, that when a student at Sandhurst, at the age of 19, he found, rather suddenly, that he could not extend the fingers when playing on the piano. During the next few weeks the paralysis advanced to its present condition. At one time he thought it was improving, but it subsequently relapsed, and had for about four years been in its present state. Mr. F—— had had syphilis, but he was certain that the paralysis had set in two years before he contracted any disease. The paralysis had not prevented his entering the army and engaging in service. He had been through the Chitral campaign.

The extensors of the carpus had no doubt escaped. There

was no dropping of the wrist, but simply inability to extend the four fingers named. No injury or sprain had ever been recognised. We may suppose that the lesion is probably of the muscle primarily, and not of the nerve-trunk.

No. LXXXVI.—*Details as to Bladder Symptoms in the early stage of Tabes.*

In the case of a man named C——, aged 34, who had suffered from syphilis nine years previously, the symptom of bladder-ache was a very definite one. He had previously seen Professor Erb in Heidelberg, who had advised him to see a surgeon and have his bladder examined. I used instruments and found neither stone nor stricture. His symptom was aching, referred to the symphysis pubis, and more or less to the whole of the pelvis. It almost always followed the act of micturition. He described it as a feeling of weakness and aching, and it had evidently distressed him much. He was much troubled by erections in the night, which were wholly unattended with sexual desire. He had in former life been very free in sexual intercourse, but of late had almost wholly abstained. None of the other symptoms of tabes were definitely marked; but he was liable to tickling sensations in his chin and upper lip, probably due to fibrillary contraction of muscles. These would sometimes precede the attacks of aching. The entire absence of symptoms of local organic disease, and the close similarity of the patient's description of his symptoms to those given by others in like condition, made me feel confident that he was in the early stage of tabes.

It should be added that the bladder symptoms had been present about eighteen months, and that previous to that he had been liable to recurring herpes on the genitals. I saw him in consultation with Dr. Lichtenberg.

No. LXXXVII.—*Liability to Epilepsy beginning in the ninth year of Syphilis—Frequent and severe attacks—Periosteal nodes—Imperfect relief from Iodides—Liability to severe pain of a peculiar character.*

The following case is an example of syphilis neglected as to treatment in its early stages, and subsequently persisting, in one or other manifestation, for many years. In the sixth year bone was lost from the nose, and the seventh, if not earlier, there were periosteal nodes on one tibia and frequent tenderness on the skull. In the eight year an epileptic paroxysm occurred, which was followed by a succession of violent attacks during the following four years. Inasmuch as during all this period there frequently occurred slight swelling and tenderness on the skull as well as a distinct node on the tibia, it is highly probable that the fits were due to the irritation of intracranial periostitis, and we may, with interest, study them in that belief.

YEAR.	AGE.	DETAILS.
1884	24	A chancre. Secondaries. Mercury given late.
1885	25	Symptoms persisted.
1886	26	Still taking medicine for various symptoms.
1887	27	Ditto ditto.
1888	28	Ditto ditto.
1889	29	Lost some bone from his nose.
1890	30	Lupoid ulcers on elbows; palmar psoriasis.
1891	31	*Came to me.* Nodes on tibiæ; thirteen stones. Soon well under iodides.
1892	32	Well for a time. In November his first convulsion.
1893	33	Returned to England. Invalided for his fits.
1894	34	Has been under G—— and B——.
1895	35	Under treatment for fits.
1896	36	Seen by me a second time. Only ten stones.

ADDITIONAL NOTES.

His first fit was in India in 1892, and was quite sudden. After this he had many and severe ones, and was sent home in 1893.

1893. In England. Saw G——. Bromides and iodides.

July. A fit after cricket; a long one, three or four hours, from which it was feared that he would not rally.

Dec. 4. Several in succession.

March, 1894. A slight attack in church.

July. Four in succession.

August and Sept. Prostrated with pains all over, in muscles, &c. *Was very ill.* (See below for more details.)

September. Saw B——. Still liable to pains. Taking iodides, &c.

Dec. 27. Another fit.

Feb., 1895. A slight fit after a long walk.

Feb., 1896. A slight fit.

April. Another. Most of the attacks have occurred in the evening.

All his fits have been sudden, without any sort of warning. "They usually come when I have been feeling unusually well."

All the recent fits have been short and slight.

August, 1896. Came to me. He has now a node on upper part of left tibia. He appears to have been all along liable to transitory nodes on the skull and left tibia, and possibly on other bones. The severe pains from which he suffered were, he thinks, not easily relieved by iodides. He says that the pain was in his muscles and bones, and was increased by the slightest touch. It was not much in his head. The liability to these pains persists, but they are less severe. Pupils active. Knee jump moderate. No form of paralysis has resulted, nor have the mental powers in any way suffered. The patient has got very thin, and looks wan and pain-worn, but is in good spirits and otherwise in fair health. He described attacks of pain which had been most severe, and which did not appear to have resembled at all closely those of tabes. The most characteristic symptoms of tabes were indeed absent. The pupils acted well. The pains had been in all parts, but especially in the limbs and back of neck, and back generally. He thought the pains had affected the muscles rather than the bones, and they had been very variable as to place, often occurring in many parts at once. They had often prevented sleep, and were described as unbearable in severity. They were almost always accompanied by tenderness, and were often caused by pressure.

It will be seen that in this case, although the history of syphilis was clear, and although there had been persisting phenomena of the malady, yet there was nothing in the character of the epilepsy which was peculiar. The attacks occurred without warning, and often when the patient was feeling in unusually good health. This is exactly what occurs in epilepsy unassociated with specific history.

The account given by the patient of the severe muscular pains from which he had suffered is worthy of note. A

comprehensive study of Pain as a symptom in Disease is a desideratum for the clinicist.

No. LXXXVIII.—*Absolute Anæsthesia of one lower extremity and lower half of trunk—Subjective Numbness of the opposite limb — Complete recovery.*

The following case is of interest on account of the unusual character of the symptoms present. Their complete disappearance supplies a valuable item of evidence in reference to prognosis.

Mr. M. L——, aged 36, was brought to me by Dr. Corbyn on October 29, 1894. He had passed through complete syphilis seventeen years previously, *i.e.*, at the age of 18. He remembered a free eruption and severe headaches, but thought that he was under treatment only from Christmas to the end of March. From that time to the date of his present illness he had remained well. Eighteen months before I saw him he had married. Three weeks before he came to me he began to suffer from pain in his back, which travelled upwards to his shoulders, and soon afterwards he found that he had lost sensation in the right lower limb. During the month previous to the occurrence of these symptoms he had been staying at the seaside, swimming every day and walking freely.

At the time of my consultation with Dr. Corbyn, October 29, 1894, we ascertained that there were no cerebral symptoms whatever. The pupils were normal and acted well. The muscular development of the lower limbs was good, and the patient could walk well. There was, however, complete anæsthesia of the right lower extremity, and side of trunk as high as the nipple. On these parts hairs might be pulled or pins thrust in without eliciting any perception. The anæsthesia involved the right half of the scrotum and penis. Mr. L—— stated that he was quite unable to appreciate the heat of water with his foot, and that he had scalded it unwittingly. The deep reflexes were good, and possibly rather excessive, in the affected limb. No anæsthesia

could be proved by pricking, &c., in the opposite limb, but the patient complained that, to him, it felt numb on the front of the thigh and leg down to the great toe. He told us also that he had had some difficulty in beginning micturition during the last three weeks, and that he had noticed that he sometimes gave a lurch in walking. Dr. Corbyn had already commenced treatment with iodide of potassium and bichloride of mercury in small doses. The pain in the back had already disappeared.

We took into account in this case that the symptoms had followed a holiday, attended probably by an unwonted amount of walking, and that the man had not long been married. He was cautioned that he must give his spinal system rest for the present, and mercury and iodides in fuller doses were ordered. In April, 1895, I had the pleasure of learning from Dr. Corbyn that recovery had been complete.

TERATOLOGY.

(*Continued from page* 273.)

No. VI.—*Spina Bifida in the dorsal region, and associated with delayed Ossification of the Cranial Bones—Diagnosis of Intra-uterine and arrested Hydrocephalus.*

An example of a very exceptional form of spina bifida was brought under my notice by Dr. R. A. Bindley, of Harlesden, in September last. Its chief peculiarities were that the tumour was placed over the lower part of the dorsal spine, and that it was coincident with delayed ossification of the cranial bones, but there were also other minor features of great interest. The infant, a boy, was nine weeks old when I saw it. The peculiar shape of its head attracted immediate attention, for there were shallow symmetrical depressions where the frontal eminences should have been, and others less marked on the sides of the skull a little behind the coronal suture. The head was probably somewhat smaller than natural, but not much, and there was nothing very noticeable in the face. On further examination it appeared that the seeming depressions were really due to bulging from within at the sutures. The frontal bone was divided down its middle, the two fontanelles were continuous, and the skull was thus open from a little above the root of the nose to the upper border of the occipital bone. This line was crossed transversely by one which occupied the whole length of the coronal suture. The bulging at these lines was just sufficient to make the adjacent parts look flat, or a little depressed, but not more; and as the head was small

rather than otherwise there was nothing to suggest hydrocephalus. Yet I may here remark that the condition is not easily accounted for on any other hypothesis than that intra-uterine hydrocephalus did exist and prevented approximation of the bones. It will be seen from what has now to be stated as to the condition of the spinal tumour that this explanation derived support therefrom.

The spina bifida was attended by a large flaccid and somewhat folded mass as big as a fist, placed over the middle of the child's back. The tumour had a broad base, around which, in parts, a considerable quantity of long soft hair, like that on the scalp, grew. It was not in the least tense, and it was not easy to be sure either of fluctuation or of impulse when the child cried. Obviously the greater part of the mass consisted of thickened skin and lobes of fat. There were one or two pendulous and pedunculated lobes exactly like molluscum pendulum. On its upper part, however, there was a thin scar, and at one spot healing was not complete. At the lower part of the base of the tumour the finger could be pushed into an opening in the spinal canal, but the upper part of this opening was protected by large everted lips of bone attached to the laminæ. As to the state of the child's lower extremities it was difficult to feel certain. They were habitually kept flexed at the hips and knees, and they were very thin, more especially on the fronts of the thighs. There was no definite talipes calcaneus, and although it might be suspected that the quadriceps extensor was on both thighs defective, I could not feel sure of it.

The explanation of this case, so far as its chief features are concerned, is not, I think, far to seek. It is an example of intra-uterine dropsy of the central canal of the cord and of the ventricles of the brain, which was in process of spontaneous cure at the time of the child's birth. Probably there had existed an opening into the spinal sac which, under the aseptic conditions of intra-uterine existence, had permitted the fluid to drain away from the cavities. At a former period probably the cranium was large and the spinal sac distended, but both were in process of contraction at the time of birth.

No. VII.—*Congenital Herpetiform pigmentation.*

Captain C——, when stripped, showed a number of brown spots on the left side of his abdomen, curving downwards and forwards exactly like a panicle of zoster. He said that he had had them all his life.

No. VIII.—*Congenital Nævoid Hypertrophy of limb.*

A young lady of 21, tall and well-developed, showed me a very remarkable condition of her right leg. It was in a condition of congenital hypertrophy with nævoid formations. The toes and foot were considerably enlarged, and on several of the toes there were thick masses of nævoid structure. The leg itself was thick and brawny, but in several places there were soft channels as big as one's thumb, which were probably venous. They did not, however, show any blue tint, nor were they attended by the slightest elevation. The condition was of course congenital. It had of late been attended by much aching in the limb, which had led to much anxiety lest some growth should be forming. The condition of the limb was naturally a cause of great chagrin to its possessor. I was permitted only a very brief inspection of it, and it had, I believe, never been shown to any one else.

The case may be compared with several others which have been recorded by various surgeons. One such with dissection was published by Mr. Tom Smith, in which enormous dilation of venous trunks was present. The lithographs illustrating it may be seen in the Clinical Museum. In another case under my own care, and of which a portrait is shown in the museum, there was much less of nævoid growth. No two cases are exactly alike, the various tissues being involved in hypertrophy in different degrees.

MISCELLANEOUS.

No. CCXXXIV.—*Multiple subcutaneous fatty tumours.*

Mr. B——, aged about 38, came to me on account of tumours on his thighs, which he had been advised to have removed. He was anxious to avoid an operation, and hence the consultation. I found that he presented a good example of the multiple fatty tumours. The lumps of fat, none of them larger than the half a pigeon's egg flattened, were scattered over his thighs and upper parts of legs. The largest were near the trochanters. They were larger and more numerous on the left side than the right. There were a few small ones on the forearms. By stretching the skin it might easily be made manifest that they were lobulated and not, as he had been told, cysts. They had caused no inconvenience, and had been present, Mr. B—— thought, from boyhood. He felt sure that they were increasing somewhat in size. I advised that they should be let alone.

No. CCXXXV.—*Gumma in the substance of the tongue—Doubt as to inherited or acquired Syphilis.*

A married lady of 25 applied to me on account of a hard lump in the substance of her tongue not far from its front and left border. It could be felt as equally near the upper and under surface, and was as large and almost as hard as a hazel nut. She said that it had been present only about six weeks, and was already, under specific treatment, getting smaller. She had no other indications of syphilis,

PLATE CLXI.

HEBRA'S BUBO.

(The Bubo of Pruriginous Eruptions on the Legs.)

This Plate is referred to in the text, and is copied from Dr. Adams' Plate.

West, Newman

either acquired or inherited. She was anxious to have it accepted as a proof that her husband had given her syphilis, as she was seeking a divorce. In the entire absence of corroborative evidence I could only assure her that it was no proof. Although gummata in the tongue are exceedingly rare as the results of inherited syphilis, it curiously happened that the one most like that which she showed, which I have recently seen, was in a woman of more than thirty, whom I had attended in childhood for interstitial keratitis, and with whose family history I was well acquainted. In both cases the gumma was in the middle of the substance of the tongue, and did not in any way implicate the mucous membrane.

No. CCXXXVI.—*Hebra's Bubo, the Bubo of Prurigo.*

Under the name of "The Prurigo Bubo," or "Hebra's Bubo," we are accustomed to recognise a soft, flabby enlargement of the glands in Scarpa's triangle, which is met with in chronic cases of pruriginous eruptions in the legs. It is a very peculiar condition, and I have but seldom met with it under any other conditions. It is a symptom of bad significance, and eruptions which have it as a concomitant seldom get well, and may even end fatally. It implies, no doubt, that the lymphatic spaces are affected, and that infective material is in course of production. Such glands seldom or never suppurate, but may remain for years loose and unattached to the skin, giving little or no pain, but slowly increasing in size.

I was well acquainted with this peculiar bubo, and with its significance, long before I knew that Hebra had drawn attention to it. I have recently been interested to find that it had been observed even long before Hebra wrote. One of the plates appended to the second edition of Adams on Morbid Poisons shows it to perfection. The lumps are wholly below Poupart's ligament, and occupy, indeed, almost the upper third of the thigh, bulging loosely downwards. The legs of the man are seen to be covered with eruption

I have thought it worth while to have Dr. Adams's illustration of this case copied. So far as I know it is the only one published, and it may serve better than a mere description to impress the fact on the memory of the reader. As is well known, common eczema but rarely causes gland enlargements. I suspect that the scratching which pruriginous eruptions provoke has much to do in causing lymphatic irritation.

No. CCXXXVII.—*Colles' Law.*

A case which I have recently investigated (see p. 320) affords interesting facts for those who have recently endeavoured to discredit "Colles' Law." A child used as vaccinifer was the means of conveying syphilis to another infant. The vaccinee at once produced a chancre on its mother's nipple, whilst the vaccinifer had been nursed throughout without any such result.

No. CCXXXVIII.—*Pigmentation of certain regions of the skin resembling that of Acanthosis nigricans, but without papillomatosis.*

Mrs. E—— consulted me on account of anxiety about discoloration of her skin. She was of very dark complexion, but until lately of a clear skin. She told me that her mother was fair, but that her father had black hair and with it a remarkably transparent skin. For about a year she had found her neck and armpits becoming dark, and she had also during the same time somewhat failed in health. She did not consider herself seriously ill, but had lost flesh and felt weak. The whole of her neck was of a darkish tint, but the borders of her armpits were very dark indeed. She told me that her waist where her dress pressed was as dark as her armpits. There were no leucodermic patches nor any papillary growths. It was clear that the irritation of the dress had influenced the location of the pigment, and the face had not become darker than formerly. Mrs. E—— had a number of coal-black

moles on her face and shoulders, none of them larger than peas. She did not think that any of them had recently increased in size. It occurred to me that these moles might possibly be exercising an injurious influence in forming pigment, and advised her to have them destroyed.

Mrs. E—— was a lady of about 30, tall and spare. If we compare her case with what is known as Acanthosis nigricans, it will be seen that the chief difference is the absence of papillomatosis.

No. CCXXXIX.—*Leucoderma—Illustration of the influence of local injury in causing the disappearance of Pigment.*

Mr. S——, whom I saw in September, 1883, was the subject of leucoderma in symmetrical patches on the two hands. A curious point was that on the back of the left thumb were two very white patches more conspicuous than the rest. One of them was quite round and as large as a sixpence. It was on the site of a blister caused by a burn some years ago. The other was a long streak, and had resulted from the scratch of a pin. Both were very definite. The influence of local irritation in producing either pigmentation or bleaching may be curiously reversed. Sometimes the one is caused and sometimes the other.

Ringworm, and more especially alopecia areata, are often followed by the growth of white hair on the patch. The same sometimes occurs over the whole head after pityriasis rubra and other severe affections causing the hair to fall. Now and then, however, instead of whitening, the hair grows blacker than before. Most scars are entirely without pigment, although their borders may be dark; and if the skin around small scars becomes deepened in tint, the latter by contrast may become very conspicuous. This often occurs on the shoulders with the scars of acne. Probably much depends upon the state of health and proclivities of the individual at the time. In the case above mentioned the tendency was to leucoderma, and it became most marked in parts which had been previously injured.

No. CCXL.—"*Stuck-finger.*"

I have just seen another example of this. (*See* ARCHIVES, Vol. VI.) It is the middle finger of the right hand and the first phalangeal joint which are affected. It slips down and is "stuck," so that it needs sometimes the use of the other hand to put it back. The patient is a rheumatic man of about 50.

No. CCXLI.—*A Note on Quinseys.*

Mr. F. M——, æt. 26, who has had many bad quinseys, tells me that they always begin with a bad cold. "I feel stiff all over as if I had got cold, and then in two or three days a quinsey forms." Most usually only one side is affected at first, and a few days later the other. Some years ago he used to have a quinsey every spring and fall, then he was five years without any, and recently he has had three attacks within six months (whilst taking iodide for syphilis).

No. CCXLII.—*Pendulous Mammæ in a young woman.*

The tendency to pendulous breasts, which is almost universal amongst savage nations, is occasionally seen in very marked degree in English women. An example of it was sent to me from a country town not long ago. Miss H——, aged 22, is a well-grown girl of a somewhat unhappy disposition. She is rather thin, and has of late lost flesh. Her breasts are large and so pendulous that when not supported they hang as low as the level of her navel. They are not fat and plump, and their lobules, slightly indurated, can be distinctly felt. She complains that the breasts ache at times. They had indeed caused her so much annoyance that the question of their removal had been entertained. Menstruation had been fairly regular. I was told that the breasts had formerly been much larger than they were when I saw them. A condition of defective tone in the integument and areolar tissue appeared to be a considerable factor in permitting the pendulosity. Breasts much heavier than

PLATE CXXXIV.

INTUSSUSCEPTION.

THE uppermost of these two figures shows a portion of intestine which had sloughed off in a case of intussusception, the patient afterwards making a good recovery. It is copied from an illustration given by Mr. John Fox, of Cerne Abbas, in an old volume of the 'Transactions of the British Medical Association.' The patient was a lad of sixteen. His illness had lasted fourteen days when the portion of gut was passed. The symptoms had differed a little from what was usual in that there had never been any bloody mucus observed in the stools. Flatus had been passed on the sixth day, and had been followed by copious motions. The treatment had been by purgatives and bleeding. The bowel, as shown in the drawing, is turned inside out. (Some additional details are given in the text, page 381.) Most of our pathological museums contain specimens of this kind.

The lower drawing is from one made by Mr. Clift for Sir Thomas Blizard, and published by him in the first volume of the 'Medico-Chirurgical Transactions.' It is of some historical interest as being probably the first delineation of an intussusception in which the gut had passed within reach of the anus. The patient was a child of five months, and death took place on the fifth day of symptoms. Six inches of the ileum with the whole of the cæcum and ascending colon had passed into the descending colon and rectum. The drawing may be compared with that given in Plate XLIX. of my 'Smaller Atlas,' which shows an almost exactly similar state of ings.

those of Miss H——'s are often seen well in place on the chest wall.

No. CCXLIII.—*Sloughing of the Intestine in Intussusception.*

Many cases are on record in which considerable portions of intussuscepted bowel have been separated by sloughing and discharged per anum. The improved surgery of the present day will no doubt make them much more rare. I have thought it worth while to copy an old plate showing the size of the portion of gut which was detached, and at the same time to state the principal facts of the case. The patient was a lad of sixteen. The following are the particulars of the case:—

Sept. 10.	Monday.	Weakly the day before, but only seriously ill to-day.
,, 11.	Tues.	A sleepless night, pulse 96. Vomiting. Pain at navel.
,, 12.	Wed.	Pain constant. Severe in paroxysms. Vomiting.
,, 13.	Thurs.	Injections returned unstained. Venesections.
,, 14.	Friday.	Vomiting continuous.
,, 15.	Sat.	Much the same. Injections of gruel and opium.
,, 16.	Sunday.	Thought to be dying. Inflation tried for first time.
,, 17.	Monday.	No sickness. Six copious stools.
,, 18.	Tues.	Improving. Copious stools, pulse 110.
,, 19.	Wed.	Slowly improving.
,, 20.	Thurs.	Improving. Bowels acting.
,, 21.	Friday.	Improving.
,, 22.	Sat.	Not so well, pulse 120. Tongue dry.
,, 23.	Sunday.	Bowels acted and portion of gut voided.
,, 24.	Monday.	Better and more comfortable, but skin still hot and dry.

After this date the improvement was steady. In a fortnight he was able to leave his bed and in a month was in good health. Four months later he was reported at his usual work and free from inconvenience. It is to be noted in this case that the characteristic symptom of intussusception—bloody mucus in the stools—did not occur, possibly because the strangulation was too tight. It is clear that something occurred on the sixth day which made the canal patent and allowed the escape of flatus. Yet the sloughed portion of gut did not come away till the thirteenth day; the bowels having been acting freely for a week. No doubt it had

been retained by some portion not detached. Purgatives had been used throughout and bleeding had been repeatedly resorted to.

No. CCXLIV.—*Hereditas Domini.*

In the Hague Gallery there is a family painting representing a father in the middle and five children in separate ovals around him. It is by Adrian Hanneman, and bears the inscription, "ECCE HEREDITAS DOMINI." No mother is portrayed or suggested.

It is recorded that an old Welsh gentleman presenting himself as a juror at an assize in the reign of Henry VIII. responded to the name of Thomas ap William Ap Thomas, Ap Richard, Ap Hoel, Ap Evan, &c. He thus took careful cognizance of his paternal predecessors, but wholly neglected his equally important maternal ones.

The student of hereditary tendencies must be often astonished at the proneness shown by observers to forget or ignore our double parentage. Those who give us what they consider facts constantly refer only to one parent—it may be the mother, but more usually the father. If the other half of the descent be recognised at all, it is usually in a half-hearted way, as if it were hardly worth mentioning. Our practice of compelling a wife to take her husband's name and put wholly aside her own, and of giving to all the children that of their father only, is to a large extent responsible for this. Long ago I suggested as a partial remedy that it should become customary to always append the mother's surname to that of her husband in those of their children—thus, John Jeffrey-Milton, William Arden-Shakespeare. A feeble attempt in this direction is not infrequently made by appreciative husbands, and one child in the family is permitted to perpetuate his mother's maiden name; but it rarely extends to more than this. My proposal is that every child should have the double name. The objection, of course, is that it could not conveniently be carried forward to more than one generation, as the names would become hopelessly cumbrous. It would, however, be a matter of great social convenience as far as it went, and it

would serve also to educate the public to a more just recognition of the importance of maternal descent.

In the case of the lower animals, the tendency is often in the reverse direction. The mother is for long closely associated with her offspring, and the father, often quite unknown, is ignored and forgotten. The creed of many children is probably that which a little girl once avowed to me, that all dogs are males and all cats females. With ladies of all ages, and for obvious reasons, the mother cat alone receives all the credit and all the responsibility for the characters of the offspring which she has reared. Her husband has never in any way claimed the custody of his children, and his existence is never thought of. A lady related to me, as an instance of education by imitation, that all the cats in her village were taking to the habit of following their owners about like dogs. "Formerly," she said, "there was but one cat, old Tom, who used to follow us. He has been long dead, but he seems to have taught them, and now they teach each other; almost all our cats do it more or less. You will say it is inheritance, but I am sure it is not, for the kittens have different mothers and are no relations." Of course I said nothing to enlighten my fair informant.

Not long afterwards a converse illustration, a splendid example of Hereditas Domini, came under notice. A Yorkshire farmer brought his son, a young man of three and twenty, to me for iritis. "Do you inherit gout?" I asked. "Not a bit of it," promptly interposed his father; "none of that in the family." "But," suggested the son, "Uncle John, you know, is often laid up with the gout." "Yes, yes, that may be; but he's no relation to me—he's your mother's brother."

No. CCXLV.—*Giants in two generations.*

Maximin, the Goth who in the third century aspired to be Emperor of Rome, is reputed to have been 8 feet high and able to tire a horse on foot or to break its leg by a blow of his fist. His son was also a giant, but "beautiful as Apollo."

CATECHISM AND CONVERSATIONS.

On Sibbens and Yaws.

(A CONVERSATION.)

Ille. You think it important to come to a conclusion as to whether the old sibbens and button scurvy of the Scotch and Irish peasantry were really syphilis or not?

Ego. I do indeed. No one can approach the question of yaws with any hope of success who has not made up his mind on these points. Some modern observers, admitting the similarity of the symptoms in these maladies with yaws, boldly go back to the old idea and say that they were not syphilis, but yaws.

I. I had supposed that it was generally admitted that they were syphilis.

E. So did I. I have seen no recent controversy on the point, but Dr. Kynsey, foreseeing, I suppose, that yaws must go with them, claims Sibbens, Morula, and Radysyge all as Frambœsia—in other words, yaws.

I. On what evidence do you rely to disprove this view?

E. Let us take Morula first. I hope you have read my account of Dr. Wallace's paper, or, still better, the paper itself. Can you believe that the eruption which he described, one which occurred to young men and women, and yielded to nothing but mercury, was anything other than syphilis? If it were something different, why do not Dublin observers meet with cases now? There was no reason why they should occur to Dr. Wallace only. If they were yaws, why were they not contagious? Every one asserts that yaws is very contagious and is communicated by contagion only. Had these "morula" cases been brought before a clinical society and well scrutinised, I have no doubt that they would at once have been declared to be syphilis and nothing else. The author gives no sort of proof

that he made an exhaustive examination. He appears, as was too much the habit in the olden time, to have trusted to what his patients told him.

I. You must admit that the necessity for mercury in order to cure is no real proof that the eruption was syphilitic.

E. I will go a certain distance, but not too far, in that direction. If a man says of a dozen cases that in all the eruption persisted so long as nothing was done, and disappeared at once when mercury was prescribed, I should certainly regard it as very strong evidence that such eruption was syphilitic. I know of no other form of eruption which always yields in this definite manner.

I. If, then, I abandon Dr. Wallace's morula cases as having been probably syphilis, still how about the others?

E. I will take "Frambœsia Cromwelliana," a disease which was left amongst the Scotch peasantry by Cromwell's soldiers. Who can doubt that it was syphilis? If it was, then was also sibbens, which was exactly like it. On the hypothesis that these maladies were not syphilis, how do you explain their disappearance?

I. Might we not attribute it to improvement in cleanliness and in food? They have yielded to advancing civilisation.

E. You forget that they were contagious maladies which went through families and affected whole villages. Nothing is more generally acknowledged than that they, like yaws, spread by contagion only. Now, crowded bedrooms and use of the same table utensils will undoubtedly increase the risks of contagion of such a disease as syphilis, but dirt and poor food have no power in causing it. They were not diseases of debility or in any way connected with poverty, excepting that poverty implies increased exposure to risks of contagion; they were contagious and specific diseases.

I. You feel confident that they were specific?

E. If they were yaws, then most certainly. Why, a slave who had gone through yaws was increased in value, like a dog that had got over the distemper, and the last news is that Fiji mothers have been accustomed to inten-

tionally give their children yaws, because, like measles, it is better to get it over in childhood. Can you doubt that such a malady is due to a specific poison—belongs, in fact, to the class of contagious fevers?

I. Your argument is that these maladies were either syphilis or frambœsia (yaws)?

E. Yes; and no one asserts anything else. If I convince you that these were nothing but syphilis, I have got a certain way towards the acceptance of the view that yaws also is syphilis, for the statements respecting the two were and are very similar, but I have not proved the point. It is possible that there may be in the tropics an eruptive febrile disease distinct from sibbens and not syphilis.

I. Is it not possible that there may be two forms of syphilis, so to speak, or two maladies not identical, but attended by very similar phenomena, as are, for instance, measles and German measles.

E. The facts seem to me to favour rather the hypothesis that we have in yaws the ancestral form of syphilis, and that the latter has become modified by long transmission through mixed races of men. If we take your suggestion that they are similar but wholly distinct maladies, like measles and German measles, then why should we not have yaws imported into England and becoming prevalent in white populations? All assert that whites are not immune, yet no single case ever comes to Europe. Why should we not, for instance, have it occurring now and then in a ship's crew? That it is almost exclusively confined to the dark races is, I think, evident, for although all assert that whites may suffer, I can find but exceedingly few recorded cases.

I. You will have read what Drs. Daniels and Wallbridge have said as to the absence of polymorphism in yaws. They regard it as the chief distinction between syphilis and yaws. They assert the sameness of the secondary eruption in all cases.

E. Yes; I have read it with great interest. You will see that there are several fallacies. The first is that if a polymorphic eruption were witnessed, the disease would be at

once diagnosed as syphilis. The next is that syphilis is by no means always polymorphic. It is remarkable how not infrequently the eruption keeps to one type. I have recorded two cases in which the eruption was bullous, a well-characterised pemphigus, and remained so throughout. The varioloid eruption of syphilis keeps to type definitely; so also does the frambœsial form of syphilis. It may be that mixture of race, and with it the development of idiosyncrasy, has had something to do with the production of polymorphism in syphilis, and that amongst an unmixed population, such as that of the Fiji Islands, there is a much greater sameness in constitutional tendencies, and hence a more uniform action of a specific animal poison.

I. You have, of course, read Dr. Pye Smith's concise and clear statement of the evidence against the syphilitic nature of yaws?

E. Certainly. His judicial mind has summed up the case with, as you say, admirable clearness, and as it stands his statement is very convincing. As is, however, now and then the case with other judicial averments, I think I can detect some flaws in his argument.

I. What do you say to his first—that yaws is never inherited, and therefore differs from syphilis?

E. It is largely a disease of young persons, and is over long before the childbearing period. This is one point, but the chief fallacy is that if an adult in a yaws district bore a child which showed symptoms of inheritance, it would at once be diagnosed as syphilis, and not yaws. The two things are as like as two peas, and through the whole story runs the fallacy that the observer may name any one case according to his predilections.

I. His next objection is that yaws does not affect the internal organs.

E. The same fallacy underlies this. If a patient had gummata in his liver or testicles they would probably be attributed to syphilis, for many patients who exhibit yaws are supposed to have had syphilis also, and the phenomena of the two diseases, assuming them to be different, are inextricably mixed. You must remember, besides, that the

exemption of the internal organs in cases of yaws is by no means proved. Very little has as yet been done in pathological search of this kind in the habitats of yaws.

I. Dr. Pye Smith's third allegation is that the eruption is not multiform, whereas that of syphilis is usually so.

E. I have already dealt with this question of polymorphism. My allegation is that syphilis is not always polymorphous, and that yaws is so to a greater or less extent. Different observers have differed a good deal in their description of its symptoms. I grant that there must be, in some shape, the raspberry-like growths, otherwise the case would not be called yaws. The diagnosis is based upon certain peculiar phenomena, and then we are told that these phenomena are always present. Of course they are.

I. To take next the statement that it is "strictly endemic."

E. This does not seem to me to amount to anything, for nobody who knows yaws attributes it to local influences; every one asserts that it is contagious, and contagious only. Its area of distribution is also very wide, and includes almost every region in which a crowded coloured population is found and where syphilis has been introduced. It is a question of race rather than of place.

I. Lastly, then, what as to the objection that yaws is not benefited by mercury?

E. The reply is that one-half of the observers, Numa Rat amongst them, assert that mercury cures it. From the earliest times of its observation there have been those who said so. The mercurialists and non-mercurialists have fought out their battle in yaws as in syphilis, and with precisely the same result—that most recent observers think the mineral more or less specific.

I. Dr. Pye Smith concludes his paragraph with the statement that the facts alleged "seem conclusive that it is not syphilis—a disease which, after all, is common in tropical climates and among other than European races, and is thereby no more 'modified' than small-pox or pneumonia." What say you to that?

E. Say? Why, I say that it is simply begging the

whole question. My argument is that syphilis is—not alone in the tropics, but in Europe also—prone to receive certain modifications by race, climate, and idiosyncrasy, and that the phenomena of yaws may possibly be one of those modifications. My friend simply declares that it never is modified, and there the matter rests so far as he and I are concerned. When he further goes on to take for granted that neither small-pox nor pneumonia are ever modified, I think he goes considerably beyond the range of knowledge.

I. What modifications do you suppose it possible that such a disease as small-pox—a specific fever—could receive from race?

E. I can imagine that it might vary much in severity, that the proportion of confluent and of hæmorrhagic cases, and of those called "malignant," might vary much. It might be also that some races would show far deeper pock-marks, as evidence of tendency to ulcerate.

I. Then you do not think the more essential characters of the disease—the duration of its stages, for instance—would be altered?

E. No, probably not. Most likely the fourteen days' period of incubation would still be observed, and so with the other stages.

I. I ask for information, but how is it with the duration of the stages in yaws?

E. They are exactly those of syphilis. This has been made the subject, not only of observations, but of experiment. Inoculation of the yaws secretion has shown that the period of incubation (before the appearance of the sore) and the interval between the appearance of the sore and the development of a general eruption are precisely those of syphilis. About this there can be no dispute.

I. Where, then, does the modification come in?

E. In the character of sore (chancre?) and in that of the eruption, both of which become framboesial, or raspberry-like, in character.

I. Are there no other modifications?

E. Yes; some observers say that the mucous membranes are not affected in yaws, and that there is no sore throat.

Others, however, assert the contrary, and Professor Breda, in particular, has figured sores on the tongue, lips, palate, &c., exactly like those of syphilis.

I. Do you, then, believe that yaws is only a name for certain peculiar manifestations of syphilis?

E. Were it not for the evidence afforded by Dr. Charlouis and Dr. Powell I should have almost inclined to avow such a creed. I am obliged, however, in face of the facts to which they bear testimony to suspend my judgment.

I. You do not, then, regard their facts as conclusive on the other side.

E. No. I wait till evidence of this kind accumulates, and until it has been carefully sifted by others. What they seem to prove is simply this, that those who have recently had yaws may contract syphilis and display the characteristic phenomena of the latter. Now, it is very remarkable, if this be otherwise than very exceptional, that other observers do not appear to have ever witnessed it. You will note that others have asserted that in all cases in which syphilis and yaws have been present together the latter has been subsequent to the former.

On the Diagnosis of Cancer of the Tongue.

(A CONVERSATION.)

Q. Have you ever thought an ulceration cancerous which proved in the result not to have been so?

A. Yes; or at any rate I have known several get well under constitutional treatment which I had judged to be cancer. I particularly remember one in which, with deep, ragged ulceration of the tongue, there was extensive implication of the glands. The diagnosis lay between a broken-down gumma and cancer, and the glands decided my opinion. Had the tongue stood alone I should have thought it a gumma. It was hopeless to operate, and so I prescribed iodides, and the man got well.

Q. This was an example of a broken-down gumma; but

have you ever thought surface ulcers cancerous which proved to be only inflammatory?

A. I have many times felt in doubt, but I do not know of any one in which I really went the length of advising an operation in which the patient got well without. There may, however, easily have been such in which the result has never come to my knowledge.

Q. Some of these may possibly have been in the precancerous stage, and may have owed their cure to local treatment?

A. It is very possible. I always apply nitric acid very freely in doubtful cases, and it may not improbably have cured some which were really in the early stage. Sir James Paget will not, I feel sure, object to my mentioning his name in connection with a case in which we both thought an ulcer on the side of the tongue cancerous and advised its excision, but two liberal burnings with the acid produced sound and permanent cicatrisation. I do not feel that the diagnosis was necessarily an error.

Q. Would not the microscope have been of use in such a case?

A. I do not think so. In the first place, to cut out an adequate specimen would have been to excise the ulcer, and this was exactly what the patient wished to avoid. In the next, supposing the verdict had been negative it would have been untrustworthy, and if positive it could not have led to a better result.

Q. You do not think highly of the usefulness of the microscope in the precancerous stage?

A. I think it, on the contrary, apt to mislead and to beget false confidence. I have never in the whole course of my experience known an instance in which the microscope declared a sore to be cancerous concerning which I had had doubts, and in which the instrumental diagnosis was confirmed.

Q. In other words, you think that naked-eye appearances which justify diagnosis occur quite as early as any of which the microscope takes cognisance?

A. Just so; for I have known several cases in which the

negative results of microscopic examination were allowed to throw doubts on the diagnosis by the unassisted eye and finger, and in which the judgment of the latter was confirmed. In some of these very valuable time had been lost owing to misplaced confidence in histology.

Q. Would you, then, disuse the microscope altogether as an aid to the diagnosis of cancer of tongue?

A. Yes, excepting as a means of affording certainty to a hesitating patient or an unconvinced colleague. I would never trust its indications if they differed from the conclusion arrived at on other grounds.

Q. But surely you may leave it as a help to those who are less self-confident than yourself?

A. They are precisely those most likely to be misled. They had far better act upon the common-sense rule to cut out everything which is doubtful.

Q. This might do for superficial ulcers; but how when it involved a question of removing the whole tongue?

A. I quite admit that you have an exception in the latter; but I can scarcely imagine a case in which a week's trial of iodide of potassium would not help me more than the microscope.

Q. Do you think a week of the iodide long enough?

A. It ought to produce very definite results in the case of a gumma—not, of course, to cure it, but to cause unmistakable improvement.

Q. On what naked-eye appearances do you most rely?

A. On a certain granular condition of the surface, like cauliflower buds on a small scale and flattened down. This is never closely simulated by syphilis. The eye and the finger must, however, always help each other. Under these buds there must always be a disc of definite induration.

Q. But may not a gumma be hard?

A. It may be very hard in an early stage, but seldom is so when ulcerated and broken down. Besides, a gumma never shows the granulated surface.

PLATE LXXXVII.

LUPUS VULGARIS OF THE GUM AND PALATE.

This Plate shows delineations, from two different patients, of the ordinary conditions of lupus vulgaris when it affects the mouth. In each instance the patient was a young woman, and the subject of lupus on other parts. In the portrait representing the gums it will be seen that the fangs of the teeth have in part been laid bare, and that masses of soft, swollen tissue bulge between them. In the portrait representing the hard palate the condition is one of ulceration, with the formation of soft, fleshy tubercles.

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